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THE UNIVERSITY OF MICHIGAN

CENTER FOR HUMAN GROWTH AND DEVELOPMENT
LANGUAGE DEVELOPMENT PROGRAM

The Development of Language Functions

KLAUS F. RIEGEL (Editor)

Report Number 8
Development of Language Functions
A Research Program-Project

ED025375

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The National Institute of Child Health
and Human Development
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The University of Michigan

Center for Human Growth and Development

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Klaus F. Riegel (Editor)

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Preface

The Language Development Program of the Center for Human Growth and Development, University of Michigan, was initiated in January, 1965 and includes seven interrelated studies in psycholinguistics. The linguistic aspects of our research include grammar, semantics and phonology. Our psychological interests involve developmental studies of perception and discrimination, recognition and recall, motivation and control, and the use of computer models of behavior. A major goal of the program is to study language as it interacts with the development of cognitive and motivational factors in the individual. Another goal is the comparison of production and motor activity with comprehension and perceptual activity. These problems are being investigated primarily through the application of experimental techniques, with interpretations in terms of various psychological, linguistic and mathematical theories.

Of major concern to the investigators has been the problem of coordinating and integrating their research activities. The investigators have collaborated in some investigations and have held periodic coordinating seminars throughout the year. Since many of the investigators had planned to invite one or two consultants to their studies, it was decided that all of the proposed scientists be invited to visit at one time. It was expected that such a joint meeting would not only provide additional impetus to the coordination of the various studies, but would also provide an opportunity for a more complete and adequate representation of our program to our guests.

On October 20, 21, and 22, 1965, five sessions were held at the Inglis House in Ann Arbor, Michigan. At each of the sessions, two visitors presented and discussed research results and problems in the areas of their specialization. The chairmen, selected from our own staff, introduced the visitors and directed the discussions among all participating members. No transcripts of the discussions were obtained, but the chairmen prepared summarizing statements which, in some cases, include special contributions by participants which were submitted to the chairmen in written form after the meetings. The meeting was concluded with a summary and general discussion which is included in the report.

I would like to acknowledge the support and assistance of many persons without whom the meetings would not have been successful.

The President of the University of Michigan, Dr. Harlan Hatcher, made the facilities of the Inglis House available to us.

The Director of the Center for Human Growth and Development, Dr. Robert Moyers, and members of the Advisory Board to the Language Development Program, Drs. Harlan Bloomer, Joseph Hartsook, Wilbert McKeachie, Arthur Melton, and Gordon Peterson, helped in the preparation and supported the meetings.

Our secretary, Mrs. Janet Bachman, solved many of the administrative problems and Miss Roslyn Bienstock typed the final version of the report.

We are grateful to all the above persons, but, in particular, to the consultants whose contributions made the success of the meetings possible.

November 30, 1965

Klaus F. Riegel

CONTRIBUTORS:

Bellugi, Ursula
Center for Cognitive Studies
Harvard University

Bilodeau, Edward A.
Department of Psychology
Tulane University

Bilodeau, Ina McD.
Department of Psychology
Tulane University

Bogartz, Richard S.
Institute of Child Behavior
and Development
University of Iowa

Eriksen, Charles W.
Department of Psychology
University of Illinois

Lane, Harlan L.
Center for Research on Language
and Language Behavior
University of Michigan

Lenneberg, Eric H.
Psychological Laboratories
Harvard University

Markel, Norman N.
Department of Speech
University of Florida

Palermo, David S.
Department of Psychology
Pennsylvania State University

Slobin, Dan I.
Department of Psychology
University of California, Berkeley

Weir, Morton W.
Department of Psychology
University of Illinois

GUESTS:

Clifton, Charles
  Institute of Child Behavior
    and Development
  University of Iowa

Fishman, Michael
  National Institute of Child Health
    and Human Development
  Bethesda, Maryland

Moyers, Robert E.
  Center for Human Growth
    and Development
  University of Michigan

Reese, Hayne W.
  Department of Psychology
  University of Buffalo

Sheppard, William C. Jr.
  Department of Psychology
  University of Michigan

Several other staff members and graduate students from the University of Michigan attended the meetings.

PARTICIPANTS:

Birch, David
  Department of Psychology
  University of Michigan

Brown, Donald R.
  Department of Psychology
  University of Michigan

Gyr, John W.
  Mental Health Research Institute
  University of Michigan

Hagen, John W.
  Department of Psychology
  University of Michigan

Hass, Wilbur A.
  Department of Psychology
  University of Chicago

Martin, Edwin J.
  Department of Psychology
  University of Michigan

McNeill, David
  Department of Psychology
  University of Michigan

Pollack, Irwin
  Department of Psychology
  University of Michigan

Prins, David T.
  Department of Speech
  University of Michigan

Riegel, Klaus F.
  Department of Psychology
  University of Michigan

Sharf, Donald F.
  Department of Speech
  University of Michigan

Tikofsky, Ronald S.
  Department of Speech
  University of Michigan

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MEMORY FUNCTIONS

Wednesday morning, October 20, 1965

Edwin J. Martin (Chairman)

Richard Bogartz is rapidly becoming known for his work in experimental child psychology. His research on memory in children marks a fresh departure from the stale Ebbinghaus tradition. Aside from the model discussed in the present report he has recently published a paper on quantitative methods in the psychology of learning. Morton Weir's interests in developmental psychology ranges over the wide spectrum from behavior genetics to problem solving and probability learning. His present paper focuses on the interaction of age and memory functions in no-solution problem solving situations.

The papers of both speakers are concerned with memory functions in sequentially structured behavior and thereby provide fundamental information on which research more complex behavioral sequences, namely language, can be built. Both speakers served as consultants for Study E: Developmental Studies in Recognition and Recall (Edwin J. Martin) and for Study H: The Development of Cognitive Systems (Donald R. Brown).

Extension of a Theory of Predictive Behavior to Immediate Recall by Preschool Children

Richard S. Bogartz

In the first part of this paper I will describe a theory of young children's behavior in the binary prediction situation and present the results of one test of that theory. I will then describe an experiment which is now in progress in which the prediction situation is transposed into an immediate recall task and indicate how the theory of predictive behavior may be transposed and tested in the recall situation.

The binary prediction or two-choice guessing situation may be paradigmatically represented as a sequence of trials, the *n*th of which is shown in Fig. 1. The temporal sequence, moving from left to right, consists of a cue of negligible duration (we have used a .3 sec. buzz), a subject-determined latent interval terminated by the predictive response, PR, also taken to be of negligible duration, an experimenter-determined delay interval terminated by onset of the event being predicted, followed by offset at the end of an experimenter-determined exposure interval. Offset of the event terminates the trial and begins an experimenter-determined intertrial interval which is in turn terminated by the cure which starts the next trial. An interval which will be of special interest in the discussion to follow is the response-cue interval, the interval between the predictive response on the *n*th trial and the cue which starts the *n+1*st trial.

When four and five year old preschool children predict in a situation consisting of a sequence of such trials, and when the two events they are predicting, A and B, alternate ABAB...on successive trials, almost all children make errors. These errors do not occur randomly; instead they appear to depend upon the behavior on the previous trial. Putting this more precisely, it appears that for most children the sequence of correct responses and errors approximates a first-order Markov chain with stationary transition probabilities. This means, simply, that the probability of a correct response on a given trial appears to depend only upon whether the subject made a correct response or an error on the previous trial, and that the conditional probabilities characterizing this dependence seem to remain fairly constant over trial sequences of at least 100 trials.

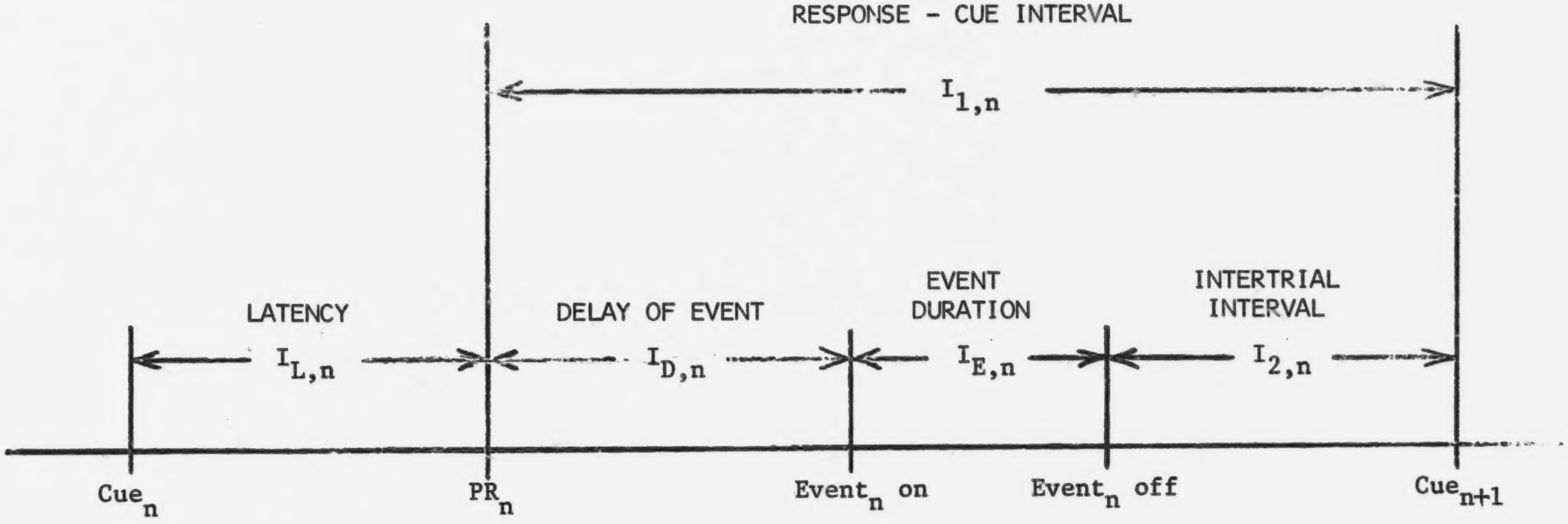

Fig. 1 Paradigmatic representation of the _n_th trial of a two-choice guessing situation.

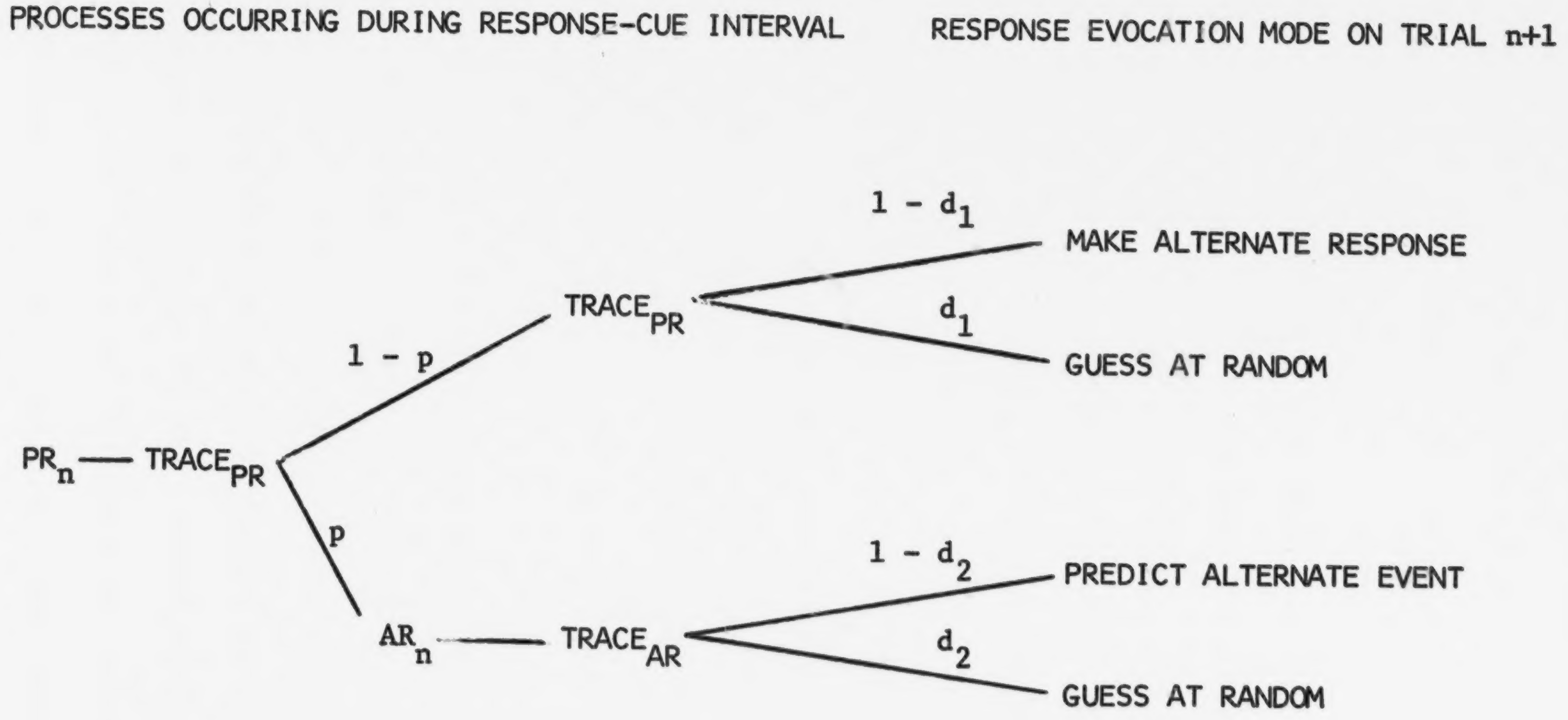

Fig. 2 Summary of the theory of predictive behavior extended to an interpretation of immediate recall.

We have observed that the children do not always seem to be paying attention to the events which they have just predicted. During the interval in which the event is exposed to them they may be looking away and not see it. Such inappropriate orientation sometimes has been observed to coincide with a run of errors in which the sequence of events alternates ABAB...while the child is predicting the events BABA...on the same trials. It seems that the child sometimes just alternates his previous response regardless of the event. We have also observed that the termination of such a run of errors is often preceded by a reorientation to the event during the last error trial, accompanied by a verbal or nonverba response suggestive of his having noticed that he has made an error.

These observations, in conjunction with Markovian properties of the data, support a theoretical formulation of prediction in the alternation situation which in everyday language can be approximated as follows. On each trial the child guesses the next event at random unless he remembers either the response he made or the event which occurred on the previous trial. If he remembers his previous response, he makes the alternate response if he remembers the event which occurred, he predicts that the alternate event will occur. In order for him to remember the event which occurred on the previous trial, he must have been paying attention to the event, and if he remembers the event, he does not remember his response.

More precisely, (see Fig. 2) it is assumed that each predictive response produces a stimulus trace which is conditioned to the alternative predictive response. If this trace lasts through the response-cue interval, it elicits the response to which it is conditioned If, however, the subject makes an attending response to the event, this response furnishes a trace which displaces the trace produced by the predictive response. If the trace produced by the attending response lasts through the intertrial interval, it elicits the response to which it is conditioned, prediction of the alternate event. Either type of trace is assumed to be vulnerable to distracting stimuli which displace the trace, and if no trace is present when the cue occurs, the subject guesses at random.

The consequence of these assumptions is that the sequence of correct responses and errors is a Markov chain with transition matrix where $\alpha = p(1-d_2)$ is the probability of

$$\begin{array}{c|cc|} & c_{n+1} & e_{n+1} \\ \hline c_n & 1 - \gamma/2 & \gamma/2 \\ e_n & \alpha + \gamma/2 & \beta + \gamma/2 \\ \hline \end{array}$$

elicitation of the response by the event trace, $\beta = (1-p)(1-d_1)$ is the probability of elicitation of the response by the trace of the previous response, and $\gamma = pd_2 + (1-p)d_1$ is the probability of guessing.

Finally, it is assumed that the conditioning states of the traces, that is, the rules which determine which response a given trace will elicit, are determined by preexperimental associations, associations established during pretraining sessions, or learning which is completed during the early trials of the experiment.

In the prediction experiment, 25 four and five year old preschool children served as subjects. Each subject was taken individually to an experimental room and seated opposite the experimenter in front of a small table upon which was a stack of 100 four by six inch white cards concealed behind a small black box which could contain the entire stack. Centered on each card was a 1.5 by 2.0 inch rectangular patch of red or green tape. The colors in the stack were ordered in a simple alternation sequence (RGRG...or GRGR...). The subject was shown the first two cards in the stack, one after the other, after being asked to name the colors on the two cards. Following correct naming of the two colors, a six-volt buzzer was sounded briefly and the child was told that each time he heard the buzzer he was to guess quickly the next color in the stack.

On each of 100 trials, following each buzz, the child made his prediction, the experimenter removed the top card from the stack, turned it color side up in front of the child for about one second, and then placed it color side down in the box. The buzzer sounded for .3 sec. every eight sec., except when the experimenter depressed a foot switch which opened the buzzer circuit. If this occurred, the interbuzz interval was increased from 7.7 secs. to 15.7 secs. A different random sequence of long and short intervals was used with each child; thus approximately half the intervals were long and half were short.

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The theory may be applied to the data both with and without inclusion of the interval variable and to group data or to that of an individual subject. When the interval variable is excluded from the analysis, we obtain the maximum likelihood estimates of α,β, and γ which you recall are the probabilities of the three response evocation modes, response alternation, event alternation, and guessing, respectively. Since these three probabilitie sum to one, only two need to be estimated from the data. With the two parameter estimates and an estimate of the initial probability of a correct response in hand, it is possible to predict for a group or for an individual subject the values of various sequential statistics which exemplify the extent to which the model summarizes aspects of the data. The sequential statistics which are shown here are: the relative frequency of response triples; the total number of runs of errors; r_j, the number of runs of errors of length j; and C(K), the number of joint occurrences of a correct response on a given trial and a correct response K trials later. There are $2^3 = 8$ response triples, one of which is observ in each group of three adjacent trials. They are: correct, correct, correct; correct, correct, error; correct, error, correct; and so on to error, error, error. A run of errors of length j is simply j consecutive errors immediately preceded and followed by at least one correct response, and the total number of error runs is simply the sum over j of the number of runs of length j. The relative frequency of the triples, the total number of error runs, the number of runs of length j, and the values of C(K) are all known functions of the parameters which I just mentioned and the number of trials in the experiment.

Table 1 shows the group observed and predicted values of the sequential statistics for all 25 subjects and for 21 of the 25. There is good agreement between the observed and predicted values, particularly in view of the fact that although the expected values of the various statistics may be obtained without inclusion of the interval variable, the variances of these statistics are increased by the interval effect. The four excluded subjects exhibited bursts of repetition of the same color prediction which were longer tha that which would be compatible with their behavior during the remainder of the trials unde the assumptions of the present theory. By repeating the same response on each of a series of trials, they made an error on every other trial, thus obtaining more error runs of lengt one than expected.

Table 1

Predicted and Observed Values of Various Group Statistics

	All 25 Subjects		21 of 25 Subjects	
Statistic	Obs.	Pred.	Obs.	Pred.
3-Tuples				
ccc	.530	.526	.501	.495
cce	.107	.109	.109	.113
cec	.085	.076	.083	.076
cee	.046	.056	.054	.063
ecc	.107	.111	.108	.113
ece	.026	.023	.030	.026
eec	.046	.057	.054	.064
eee	.053	.042	.060	.052
Mean number of runs of errors				
	13.480	13.459	14.048	14.015
Runs of errors, r_j, of length j				
r_1	8.880	7.784	8.619	7.724
r_2	2.000	3.282	2.381	3.467
r_3	1.400	1.384	1.667	1.556
r_4	.680	.583	.810	.698
r_5	.120	.246	.143	.313
Autocorrelation of correct responses, C(K), K trials apart				
C(1)	63.000	62.901	60.286	60.144
C(2)	60.320	58.981	57.238	55.905
C(3)	58.120	57.553	54.714	54.371
C(4)	57.960	56.750	54.714	53.554
C(5)	56.840	56.105	53.476	52.928

Table 2

Predicted and Observed Total Number of Runs of Errors

Subject	Observed	Predicted
1	18	17.24
2	11	11.04
3	17	15.12
4	7	3.66
5	18	18.23
6	17	17.05
7	24	21.85
8	11	10.77
9	13	12.77
10	18	18.10
11	15	14.77
12	18	15.23
13	10	10.17
14	10	10.10
15	18	17.70
16	7	7.03
17	8	8.11
18	11	10.82
19	13	13.02
20	9	4.25
21	9	9.02
22	12	5.93
23	14	14.04
24	12	11.86
25	17	17.08

Table 2 shows the observed number of error runs and predicted number of error runs for the 25 individual subjects. The theory appears to be describing well the behavior of most of the individual subjects as well as that of the group.

Application of the theory to the data with inclusion of the interval variable results in two predictions. The first, a rather weak prediction, is that the probability of guessing following the long interval should be greater than that following the short interval since we expect that the longer the interval, the less likely should it be that either type of trace will still be present and therefore the more likely it should be that the subject will guess. This prediction was confirmed in that the estimate of the probability of a guess following the long interval was .53 and the corresponding estimate for the short interval was .15.

Maximum likelihood estimates of the response evocation mode probabilities for each interval may be used to obtain a sequential statistic which provides a stronger test of the theory with respect to its applicability to the predictive situation when experimenter-controlled sequence of intervals are used. A 3,2-tuple is a joint event consisting of three predictive responses, each of which is either correct or an error, and the two intervening intervals. For example, correct response, short interval, error, long interval, error is a 3,2-tuple; likewise error, long interval, error, short interval, error. In all, there are 32 3,2-tuples which may occur if only two different intervals are used. By estimating two parameters for each interval, it is possible to predict the relative frequencies of all 32 3,2-tuples.

Table 3 shows the observed and predicted values for the 32 3,2-tuples for the data from all 25 subjects. The corresponding table for the 21 subjects showed a slightly better picture.

The data from almost all of the subjects support three implications of the model: the sequence of correct responses and errors is approximately a first order Markov chain with stationary transition probabilities; the probability of an error given a correct response on the previous trial ($\gamma/2$) is less than or equal to both the probability of a correct response given an error ($\alpha + \gamma/2$) and the probability of an error given an error ($\beta + \gamma/2$);

Table 3

Predicted and Observed 3,2-Tuples

3,2-Tuple	Obs.	Pred.	3,2-Tuple	Obs.	Pred.
c S c S c	.156	.164	c L c S c	.137	.130
c S c S e	.016	.013	c L c S e	.006	.010
c S e S c	.009	.008	c L e S c	.032	.028
c S e S e	.004	.006	c L e S e	.018	.023
c S c L c	.123	.130	c L c L c	.114	.103
c S c L e	.054	.047	c L c L e	.031	.037
c S e L c	.011	.008	c L e L c	.033	.030
c S e L e	.004	.006	c L e L e	.021	.021
e S c S c	.025	.030	e L c S c	.030	.032
e S c S e	.003	.002	e L c S e	.004	.003
e S e S c	.011	.014	e L e S c	.013	.013
e S e S e	.016	.012	e L e S e	.014	.011
e S c L c	.027	.024	e L c L c	.025	.025
e S c L e	.011	.009	e L c L e	.009	.009
e S e L c	.011	.015	e L e L c	.011	.014
e S e L e	.014	.011	e L e L e	.008	.010

and the probability of an error given a correct response is greater following the long interval than following the short interval. The data therefore also support the theory which gave rise to these predictions in which the relevant processes are conceptualized in terms of attention, event and response traces, and random guessing.

It should be observed, however, that other conceptions of the subject in terms of different processes could also give rise to the same prediction concerning these properties of the data. Thus, while the goodness of fit is certainly encouraging, its usefulness in establishing the appropriateness of the proposed concepts is limited. The value of identifying model parameters with terms such as "guessing probability" or "the probability of the response-produced trace eliciting the next response" will be established more by the scope of the theory, by its extendability to other behavioral measures and other situations, and by confirmation of predictions, suggested by these identifications, concerning the covariation of parameter values with values of independent variables.

The immediate recall task to which the theory will be extended incorporates two changes directed to delimiting the scope of the theory. Previously, the theory has been applied to prediction of the next event in various binary sequences (including alternation, noncontingent, and Markov sequences). In the recall task the subjects will postdict (recall) the previous event in the ternary sequence of colors Red-Blue-Green-Red-Blue-Green...The events will be colored lights on a panel, with a different color at each vertex of a triangle. The child will be instructed to recall the previous color each time the buzzer sounds. The session will start with a red light, a buzzer will sound, the child will respond, another light will follow, etc. A variable event-cue interval will also be used. The child's response and response latency will be recorded.

With these changes, the sequence of occurrences in the situation will now be: Event-Buzz-Response-Event-Buzz-Response-... as opposed to Buzz-Response-Event-Buzz-Response-Event... in the prediction task. Note, however, that a latecomer to the situation could not tell, even after observing several complete trials, whether he was observing a prediction task or a recall task. In other words, so far as the temporal sequence of observable events is concerned, the two situations are the same once they have been going for a while.

This congruence of the two tasks, in conjunction with the assumption that in the reca task, as in the prediction task, the subject's behavior should be affected by memory of the event, effects of his previous response, lagging of attention, and guessing, suggests at least two models of the situation in terms of the theory described above. The two diff only in the assumption concerning the conditioning state of the trace produced by the reca response. They will be referred to as Models A and B. In both models it is assumed that the absence of any trace results in a guess and that the trace of the event elicits the correct recall response.

In Model A it is assumed that the trace of the previous recall response elicits the name of the next color in the sequence. Thus, if the subject has just said "Red" and he does not pay attention to the next color shown to him, then if the trace of his response is still present when the buzzer sounds, he will say "Blue." This is the sort of assumpti to make if we expect that in this situation, as in the alternation prediction situation, the children, when not paying attention, will often give the colors in the right order but out of phase with the events.

In Model B it is assumed that the trace of the recall response elicits that same response. Thus, if the subject has just said "Red," the trace of this response will elicit "Red" on the next trial if it is still present. The idea underlying Model B is that if the children rehearse the name of the color which they have just seen until the buzzer sounds, they may, when attention lags, respond to the trace of their previous response as they would respond to the trace produced by rehearsal. This would result in repetition of their previous response.

To summarize the differences between the two models and to relate Model A to the mode used in the prediction situation, it is convenient to discriminate two kinds of errors that can occur in recall. We will say that if the subject's response is one ahead of the correct response in the sequence he has made an e_1 and if it is two ahead he has made an e_2. Thus if Red is correct, "Blue" is an e_1 and "Green" is an e_2. Then the transition matrix implied by Model A is

$$\begin{array}{c|ccc|} & c_{n+1} & e_{1,n+1} & e_{2,n+1} \\ \hline c_n & 1 - 2\gamma/3 & \gamma/3 & \gamma/3 \\ e_{1,n} & \alpha + \gamma/3 & \beta + \gamma/3 & \gamma/3 \\ e_{2,n} & \alpha + \gamma/3 & \gamma/3 & \beta + \gamma/3 \\ \hline \end{array},$$

and that implied by Model B is

$$\begin{array}{c|ccc|} & c_{n+1} & e_{1,n+1} & e_{2,n+1} \\ \hline c_n & \alpha + \gamma/3 & \gamma/3 & \beta + \gamma/3 \\ e_{1,n} & \alpha + \beta + \gamma/3 & \gamma/3 & \gamma/3 \\ e_{2,n} & \alpha + \gamma/3 & \beta + \gamma/3 & \gamma/3 \\ \hline \end{array},$$

where in both cases, as in the model for predictive behavior, α, β, and γ are the probabilities that the recall response is elicited by the trace of the event, elicited by the trace of the previous response, and is a guess, respectively. Inspection of the two transition matrices reveals that the difference in assumptions concerning the effect of the response-produced trace is reflected by different equalities and inequalities of various conditional probabilities.

An interesting property of Model A but not of Model B is that the two error states are lumpable. This means that if the type of error is ignored and the responses are simply categorized as correct or error, the sequence of c's and e's will still be a Markov chain with transition matrix

$$\begin{array}{c|cc|} & c_{n+1} & e_{n+1} \\ \hline c_n & 1 - 2\gamma/3 & 2\gamma/3 \\ c_n & \alpha + \gamma/3 & \beta + 2\gamma/3 \\ \hline \end{array}.$$

This matrix is a special case of the matrix

$$\begin{array}{c|cc|} & c_{n+1} & e_{n+1} \\ \hline c_n & 1 - (N - 1)\gamma/N & (N - 1)\gamma/N \\ c_n & \alpha + \gamma/N & \beta + (N - 1)\gamma/N \\ \hline \end{array},$$

where N is the number of events in the sequence. Setting N equal to 2 in this matrix we obtain the transition matrix implied by the prediction model. This reflects the fact that Model A is based on the same assumptions concerning the effects of the previous response on the next response as is the prediction model.

In the event that the recall model is compatible with the data, we plan to pursue the problem by (1) using probabilistic ternary sequences in prediction and recall, (2) interpolating distractive or associatively interfering events into the situation at different points in the sequence and evaluating their effects on the parameters as a function of their location in the sequence, (3) following up some very promising results which have supported a prediction concerning a conditional response latency measure which was made using the estimates of α, β,and γ to weight other conditional latencies in a prediction equation generated by the theory plus the auxiliary assumption that the means of three response latency distributions, one associated with each response evocation mode, all exist, and (4) using subjects in relatively small regions of the α-β-γparameter space in parametric studies of the relationship between certain interval durations and the α, β, and γ values.

In the event that the recall model does not fit, we shall have a lot of recall respons and latencies, and the interesting question of how the instructions to recall teh previous event transform the subject from a Markovian predictor to a non-Markovian postdictor.

Age and Memory as Factors in Problem Solving[1]

Morton W. Weir

In a recent paper (Weir, 1964), I examined the behavior of subjects ranging in age from three years to adult in a three-choice probabilistic task. The apparatus which was used is shown in Fig. 1. It consisted of a signal light centered above three push buttons, and a delivery hole for marbles centered below the three buttons. The marbles fell into an enclosed, clear plastic container. The subject was told that when the light went on, he was to press one of the three buttons. He was also told that if he pressed the "correct" button, a marble would fall into the container, and that the object of the game was to win as many marbles as possible. One of the knobs paid off part of the times it was chosen (33% or 66%), while the other two were dummies and never paid off. This constituted a partial-zero-zero reinforcement schedule, with the partial reinforcement being delivered randomly.

As was pointed out in the earlier study (Weir, 1964), it appears that subjects of all ages, with the possible exception of children below five years of age, regard this task as a problem solving situation. They generate hypotheses and employ strategies aimed at solution of the problem. The solution expected by them is almost always of a pattern-of-response nature, and subjects generally believe that if they achieve solution they will be able to win a marble on every trial, or at least predict accurately when a marble will be delivered and when it will not. Since the reinforcement schedule is random, neither of these solutions is possible.

I should like to review briefly some of the findings and conclusions of the earlier study (which I shall subsequently refer to as the "1964 study"), and to tell you of some further research on this problem which I believe demonstrates the importance of the role of memory in determining performance in this type of task.

Figure 2 presents the proportion of correct responses during the last 20 trials of this task plotted as a function of age. A correct response is defined as any time the subject chooses the only button that pays off, regardless of whether or not it actually

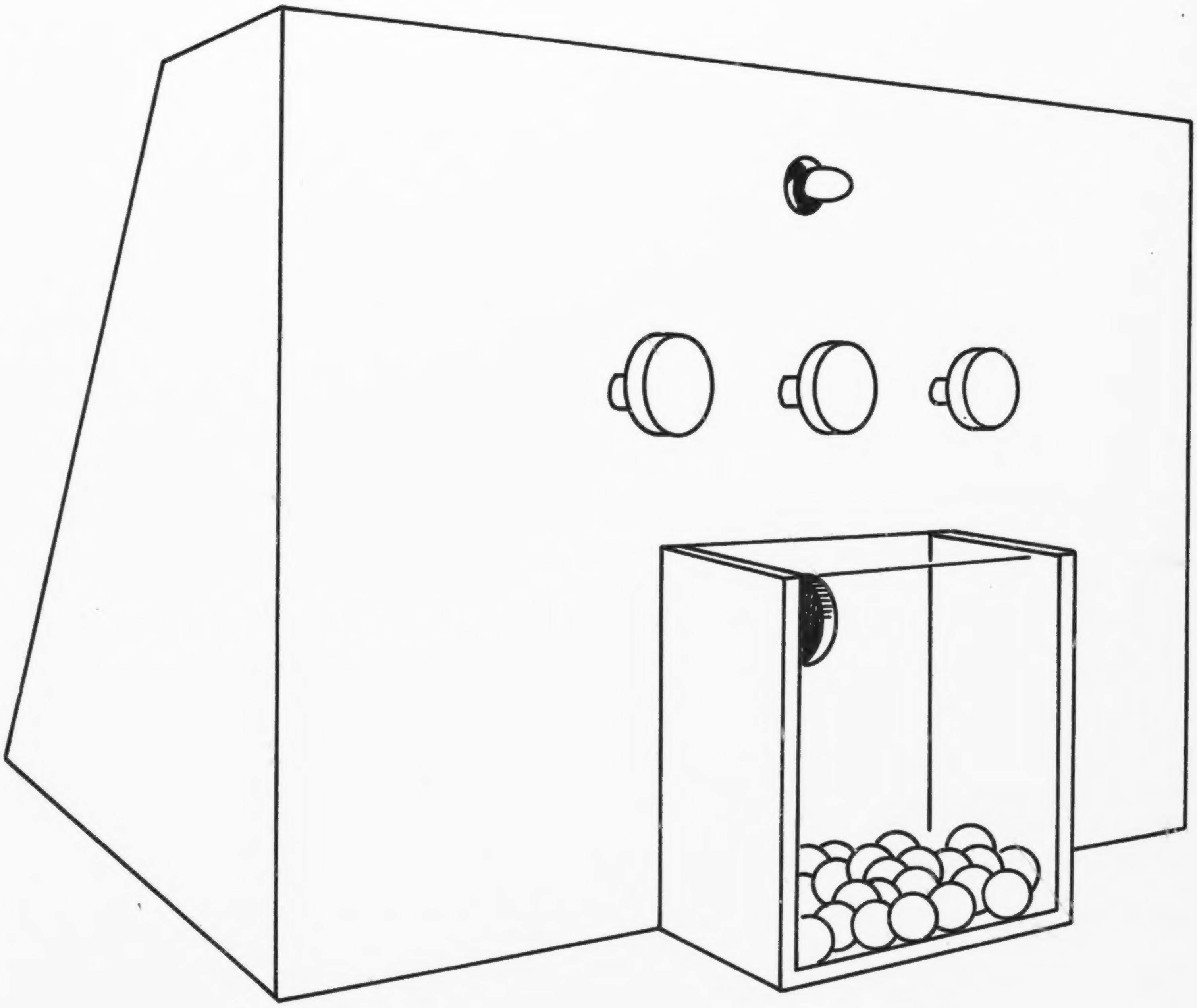


FIG. 1

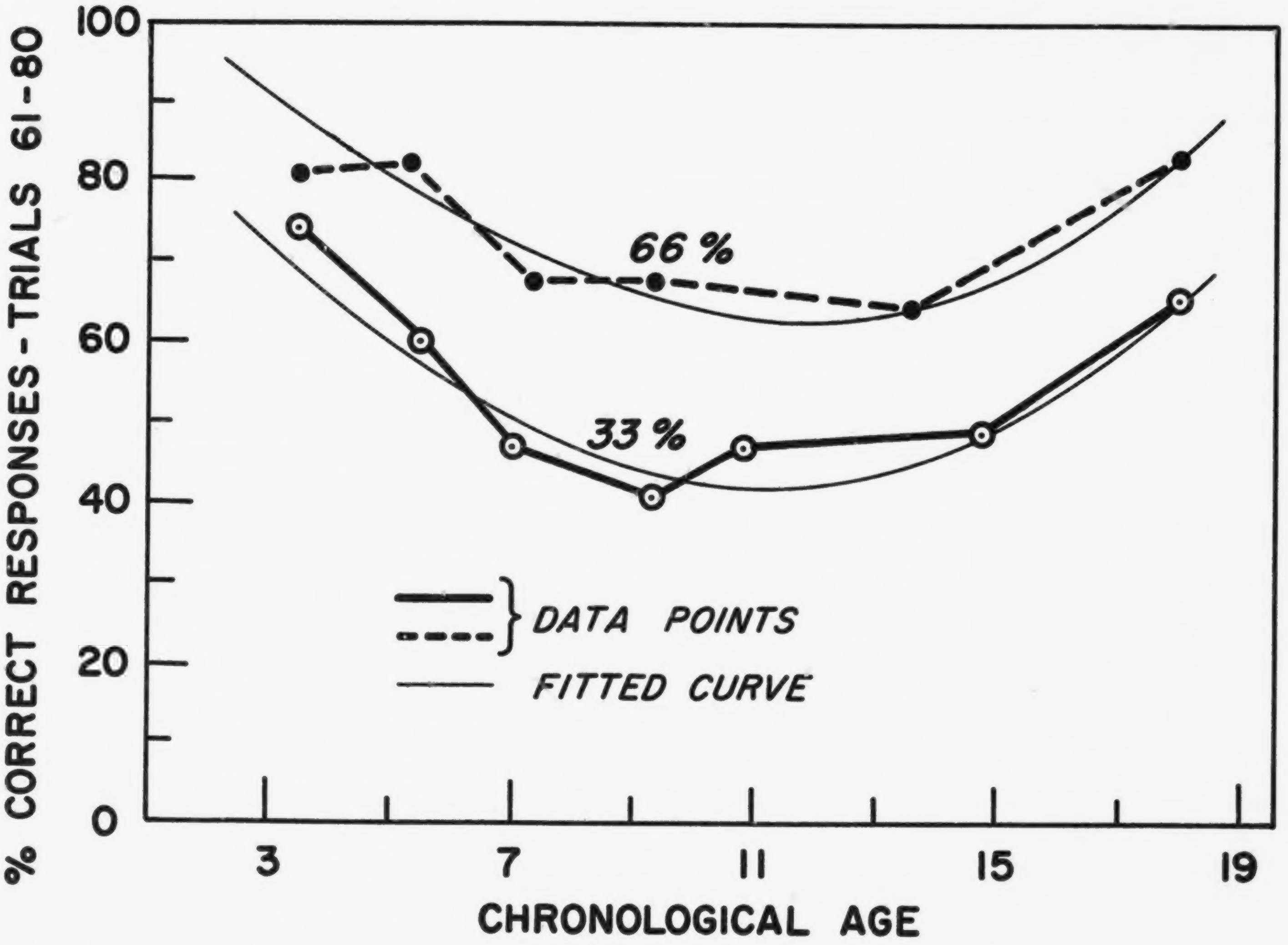

% CORRECT RESPONSES - TRIALS 61-80
100
80
60
40
20
0
3
7
11
15
19
CHRONOLOGICAL AGE
66 %
33 %
DATA POINTS
FITTED CURVE

FIG. 2

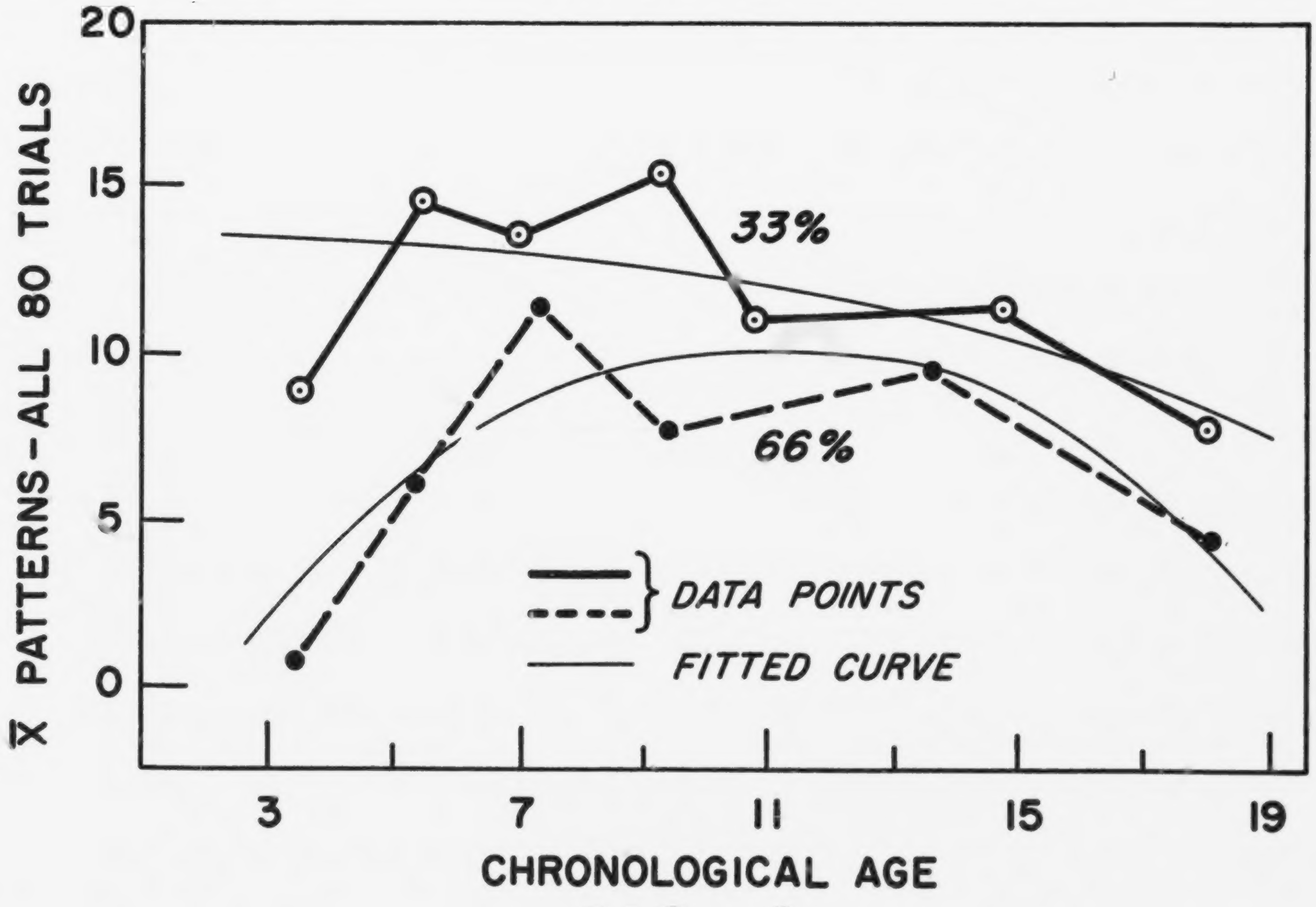

X̄ PATTERNS - ALL 80 TRIALS
20
15
10
5
0
3
7
11
15
19
CHRONOLOGICAL AGE
33%
66%
DATA POINTS
FITTED CURVE

FIG. 3

paid off on that particular trial. Note the curvilinearity of these data for both 33% and 66% reinforcement schedules. Note also that the nine to 11-year-old age range represents the lowest portion of this curve. The younger children (three to five-year-olds) tend toward maximization of their choices of the pay-off button; the same is true of adults, although learning curve data make it clear that the adults reach this high terminal level much more slowly than do three- and five-year-olds.

If the nine to 11-year-olds are not choosing the pay-off button, what are they doing? Examination of the frequency of simple patterns of response supplies at least a partial answer. Plotted in Fig. 3 are the mean number of response patterns of a left, middle, right (LMR), or a right, middle, left (RML) nature as a function of age for the two reinforcement schedules. Although these data are quite variable, it appears that children in the "middle-age" range display more of these simple patterns than do younger or older children. This is especially true of the 66% condition.

The nine to 11-year-old child, then, is certainly not responding randomly. The relatively few choices of the pay-off button which he displays occurs because he is apparently instead making a large number of these simple response patterns. As tentative explanation of these data, consider the following line of reasoning. Given a spatial task of this level of complexity, a child of age nine to 11 might very well be far enough along in his cognitive development to enable him to generate some hypotheses involving patterns of response, while a younger child might not be able to do so. It is interesting to note, however, that the nine to 11-year-old child continues to make these simple response patterns throughout the task, even though they do not consistently pay off. It is as if it is very difficult for him to reject this simple strategy, even though it does not work. In order to explain this response stereotypy, it appeared possible that although the nine to 11-year-old child may be far enough advanced in his development to generate such patterns, he may not be far enough advanced to process the information available from the results of his own responding. He may be, at this age, fairly sophisticated hypothesizer, but a poor information processer. If so, he perhaps would not be able to compile sufficient informatio to enable him to discard these ineffective simple strategies, and as a consequence he would try them often throughout the task.

In considering this hypothesis, it appeared possible that the poor information processing of the "middle-age" child, at least when compared with his ability to generate hypotheses, might be the result of an inadequate memory. He might be capable of generating LMR or RML patterns, but not be able to remember his reponses and and their outcomes more than a few trials back, get confused, and start over. If an insufficient memory is a crucial factor, then supplying a nine-year-old with a memory comparable to that of an adult should change the child's performance considerably in the direction of that displayed by older subjects. That is, the nine-year-old with a memory aid should show more frequent choice of the pay-off alternative and fewer simple patterns of response than should a nine-year-old with no aid to memory provided. In order to test this idea, a simple, but infallible memory was designed for the use of the nine-year-old. It consisted of a pegboard with many rows of three holes each. The children who were given this memory aid were instructed that each time they pressed a button, they were to put a peg in one of the three holes corresponding to the position of the button they had just pressed. On the next trial, they were to drop down a row and do the same thing. If they won a marble, they were told to place a black peg in the hole; if they did not win, a pink peg was inserted. If a subject understood the use of the peg board, he could look back over his past responses and determine exactly which patterns he had been using and what their outcomes had been. Five-year-olds, nine-year-olds, and college adults participated in this experiment. Within each age group, one-half of the subjects used the memory aid and one-half did not. Other than this, the task and instructions were the same as in the 1964 study. The same 66% reinforcment schedule was used, subjects were run for 120 trials, and of course only one button ever paid off.

The proportion of choices of the pay-off button for the memory and non-memory groups of nine-year-olds is shown in Figure 4. The result with this age group was as expected, with the memory aid condition exceeding the non-memory condition. This difference is significant at beyond the .01 level. Also note the "X" which is labeled "64-9". This represents performance of nine-year-olds as predicted from the fitted age curves derived from the 1964 study. The purpose in placing this marker on the graph is to provide some rough indication of the reliability of these data.

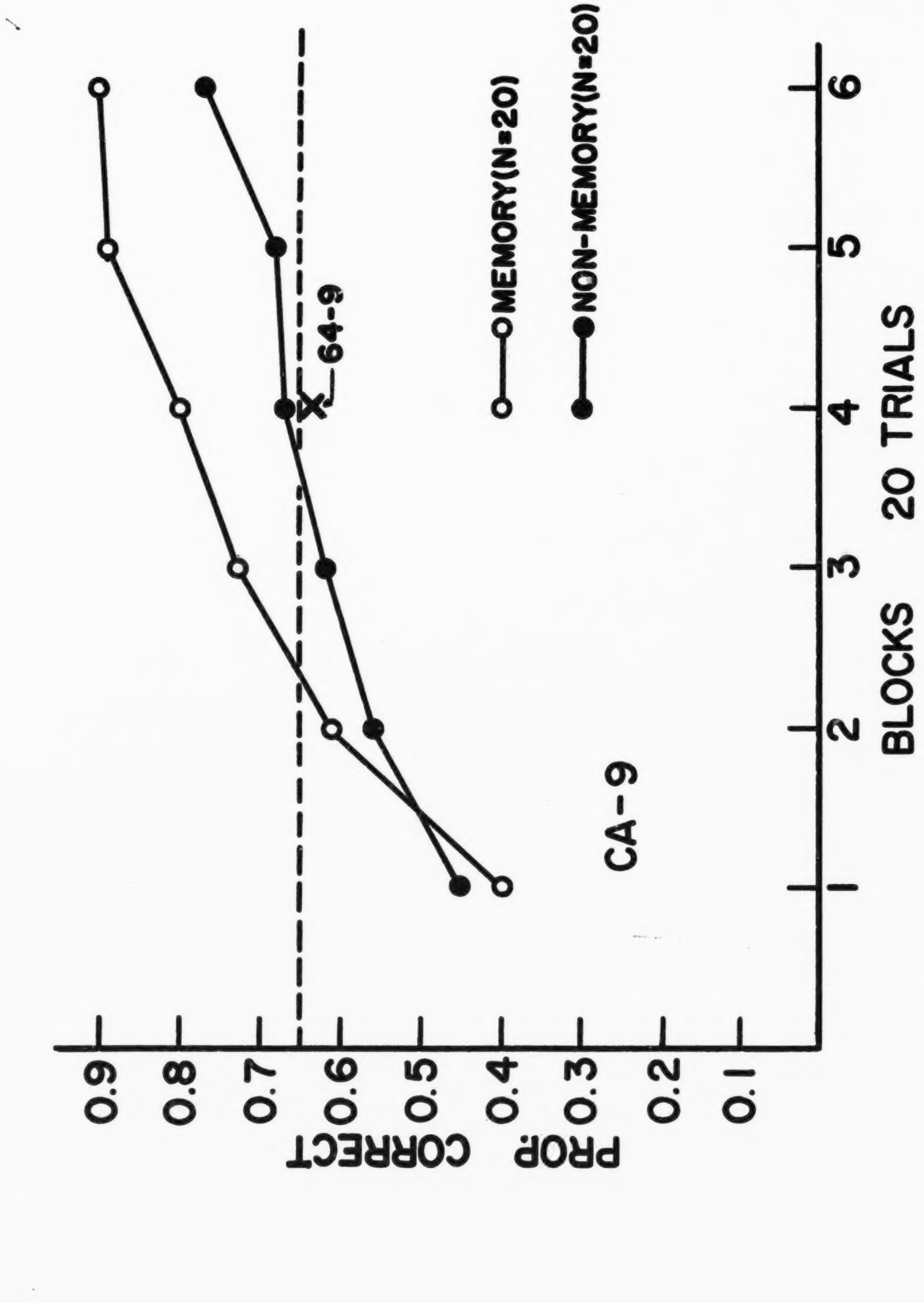

PROP CORRECT
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
1
2
3
4
5
6
BLOCKS 20 TRIALS
CA-9
64-9
MEMORY (N=20)
NON-MEMORY (N=20)

FIG. 4

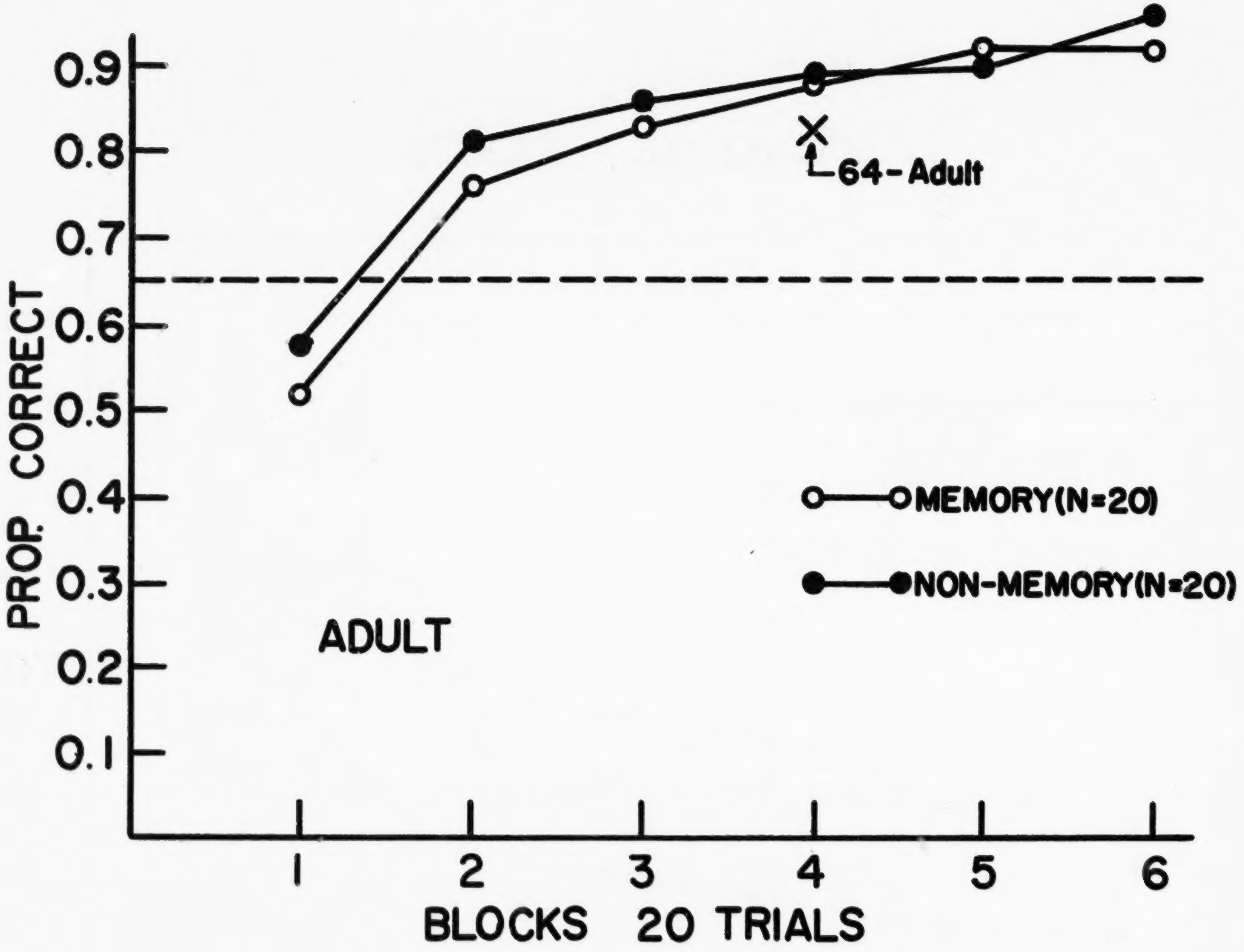

0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
PROP. CORRECT
1
2
3
4
5
6
BLOCKS 20 TRIALS
64 - Adult
ADULT
MEMORY(N=20)
NON-MEMORY(N=20)

FIG. 5

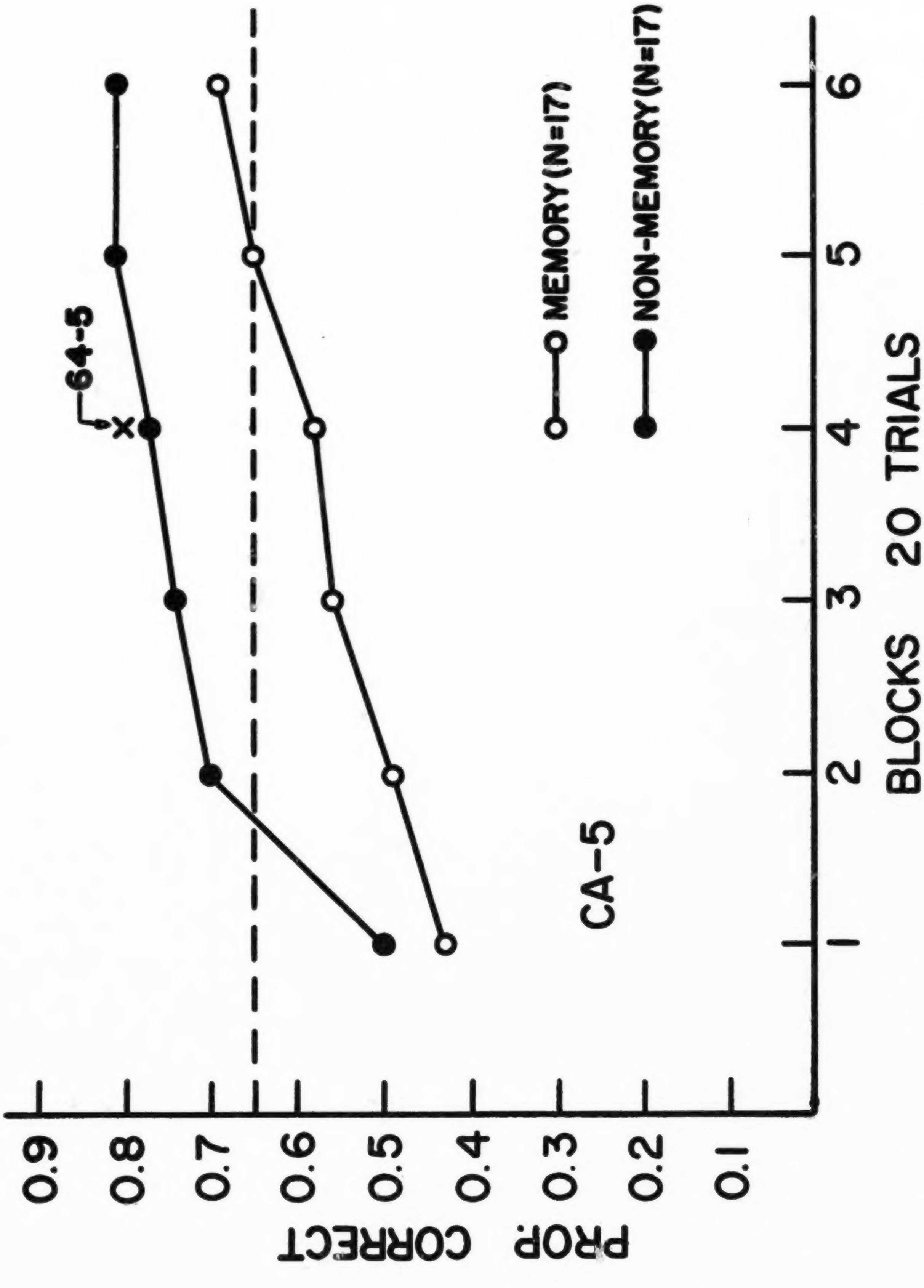

CA–5
64-5
X
PROP. CORRECT
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
1
2
3
4
5
6
BLOCKS 20 TRIALS
MEMORY (N=17)
NON-MEMORY (N=17)

FIG. 6

In Figure 5, the same data are plotted for the adult group. There is no difference between the memory and non-memory conditions, and the terminal proportion of the adult data predicted from the age curves in the 1964 study is very close to both these curves. No difference had been predicted here, as it was thought that the adults would not need the aid of this device in recalling fairly long series of trials and their outcomes, and it would therefore change their behavior little, if any, during the task.

The five-year-olds present a completely different picture. As Fig. 6 indicates, the memory aid had an effect opposite to that noted with the nine-year-olds. This difference is also significant at beyond the .01 level. In addition, the prediction from the fitted curves of the 1964 study agrees closely with that of the equivalent (non-memory) condition in this experiment. In watching these five-year-olds perform in this task, the source of this effect seemed clear. Many of the five-year-olds did not understand the relationship between the memory device and the probability task. They frequently made errors in peg placement, and often could not remember which button they had just pressed. In anticipation of this problem, all subjects in this study had a second experimenter present who sat beside and slightly behind the subject. The function of this second experimenter was to aid the subject in peg placement if he had trouble. Thus, all subjects had before them an accurate representation of past responses and their outcomes. The five-year-olds frequently needed help from this second experimenter, and it appeared as if the memory board was a completely irrelevant task interposed between trials which served to disrupt performance in the probabilistic task.

LMR and RML patterns were also examined for the three age groups, and a significant reduction in the number of such patterns occurred in the nine-year-old group, as had been predicted. There was no significant change in the number of these patterns for either the five-year-olds or the adults.

These data appear to support the original hypothesis. However, the nine-year-olds could be making another use of the memory aid besides keeping track of past response patterns and their outcomes. After 15-20 trials, it becomes obvious from examination of the pegboard that all the black pegs occur in only one column. This provides the nine-year-old with information about something which he might otherwise be confused--that only one button pays off. In fact, in questioning nine-year-olds after the task, those without a memory aid occasionally volunteered the information that the other two knobs sometimes paid off, when of course, they actually had not.

In order to assess the role of the information conveyed by the columns of the memory board, a small change was made in the apparatus. Instead of a single center delivery box for marbles, the apparatus was converted to provide three receptacles, one associated with each knob. This change should provide the same information that all the black pegs in one column of the memory board provides--that is, that only one knob pays off. With the converte apparatus, this information is conveyed to the subject by the fact that marbles only fall into one box, or, as the children put it, "out of one knob." In this task we again ran five-year-olds, nine-year-olds, and college adults, using the same 66% reinforcement schedule and a task length of 120 trials.

The results for the nine-year-olds are presented in Fig. 7. If the critical feature of the memory aid was the information in conveyed concerning the fact that only one knob ever paid off, it might be expected that subjects in the three-box condition would show an increase in performance similar to that shown by subjects in the memory board condition. As can be seen in Fig. 7, there is no difference between the three-box and one-box conditions and both of these are very similar to the non-memory condition of the previous study. The data from the nine-year-olds in the memory board study are also shown in Fig. 7 for compariso purposes.

The picture is the same for the adults, as well as the five-year-olds. The three-box condition has no effect upon performance. The results are so similar in that regard to those of the nine-year-olds, that no graphical representation seems necessary.

It therefore appears that the memory board functions as it was designed--it allows the nine-year-old to look back over a series of responses and their outcomes, perhaps as an older child or an adult would use his own memory in a task such as this. He is able to make use of this information by rejecting simple patterned strategies when they do not work (that is, do not end in a black peg), and eventually show a tendency toward maximization of choices of the only alternative which pays off. This behavior is much more adult-like

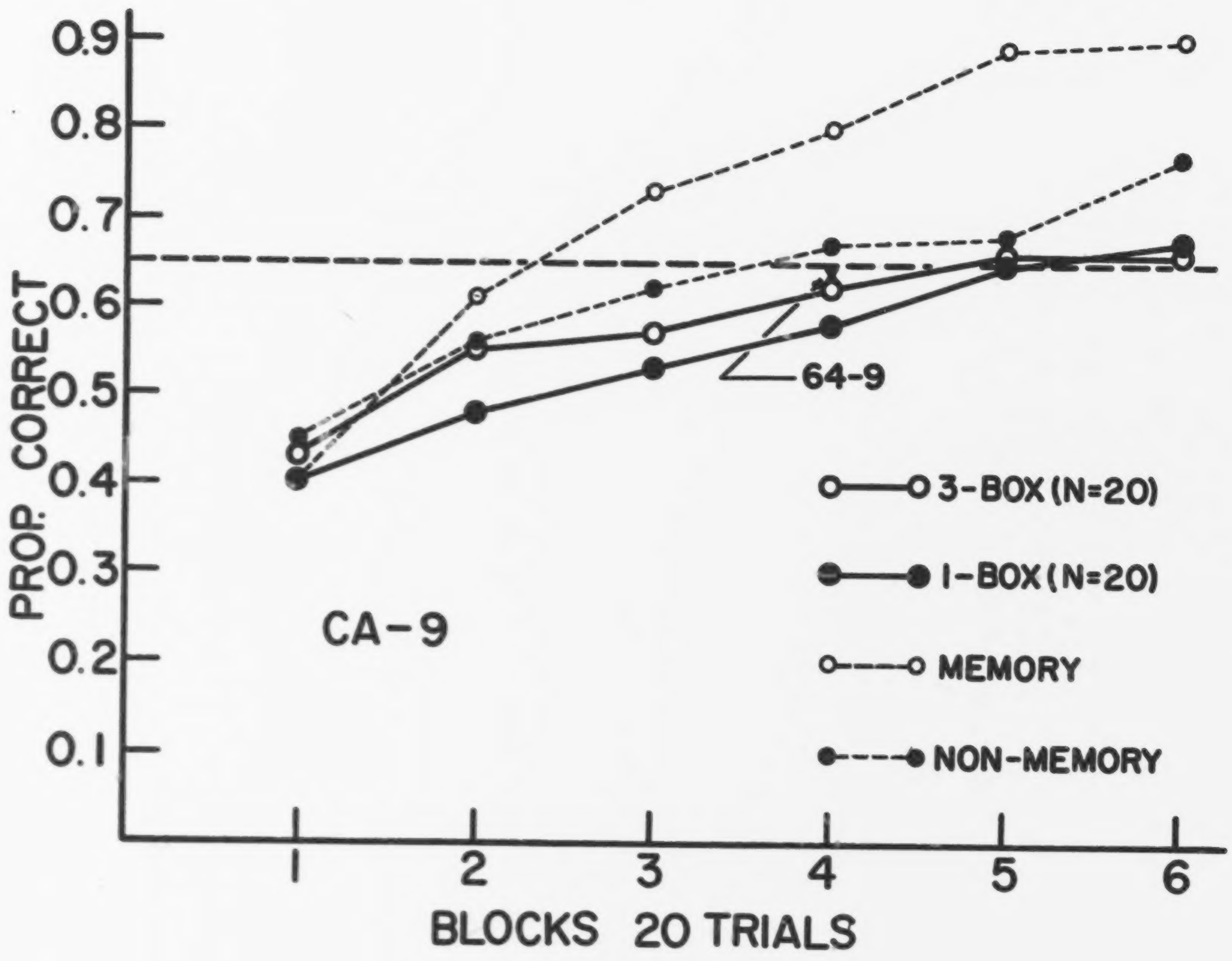

PROP. CORRECT
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
1
2
3
4
5
6
BLOCKS 20 TRIALS
CA-9
64-9
3-BOX (N=20)
1-BOX (N=20)
MEMORY
NON-MEMORY

FIG. 7

than is that of nine-year-olds without a memory aid. It thus appears that the hypothesis that the continued use of simple patterns of response seen in the nine-year-olds in the 1964 study might have been due in part to an insufficient memory gains support from these data.

The experiments which I have just described have, I believe, a direct bearing upon memory as it relates to developmental changes in problem solving strategies. I should now like to present some data which may or may not relate to memory, but which I believe are sufficiently orderly and interesting to warrant close consideration. I have a tentative interpretation of the data, but I am most interested in the reactions of the rest of the symposium members as to their interpretation.

In these studies, an apparatus was used which was very similar to that used in the probabilistic task which I have just described. The major difference was that the number of alternatives was varied, as was the reinforcement schedule. Subjects were assigned to either a two- or a four-choice condition. For some subjects, one of the two (or four) knobs paid off 70% of the times it was chosen and the other one (or three) never paid off. For other subjects, one of the alternatives paid off 70% of the times it was chosen, while the other one (or three) paid off 30% each. This resulted in the formation of four conditions, with two levels of number of alternatives, and two reinforcement schedules (i.e., 70:0, 70:0:0:0, 70:30, and 70:30:30:30). In the two conditions in which only one knob paid off, five age levels were used (4, 6, 9, 12 and adult); in the two conditions in which all alternatives paid off at least part of the time, six age levels were used (4, 5, 7, 9, 13 and adult). All subjects were run for a total of 120 trials.

The choice behavior of these subjects was then analyzed for response patterning. Rather than pick only one type of pattern, as was done in the study just described, an information analysis of response sequences was performed. In this type of analysis, a computer examines all possible patterns of run lengths of up to 10 in the two-choice task and up to six in the four-choice task, and provides a measure of the degree to which subjects are using repeated response patterns of various lengths.

Figure 8 shows the outcome of this analysis for the two-choice, 70:30 task. In this figure, uncertainty is plotted as a function of the number of responses in sequence for the

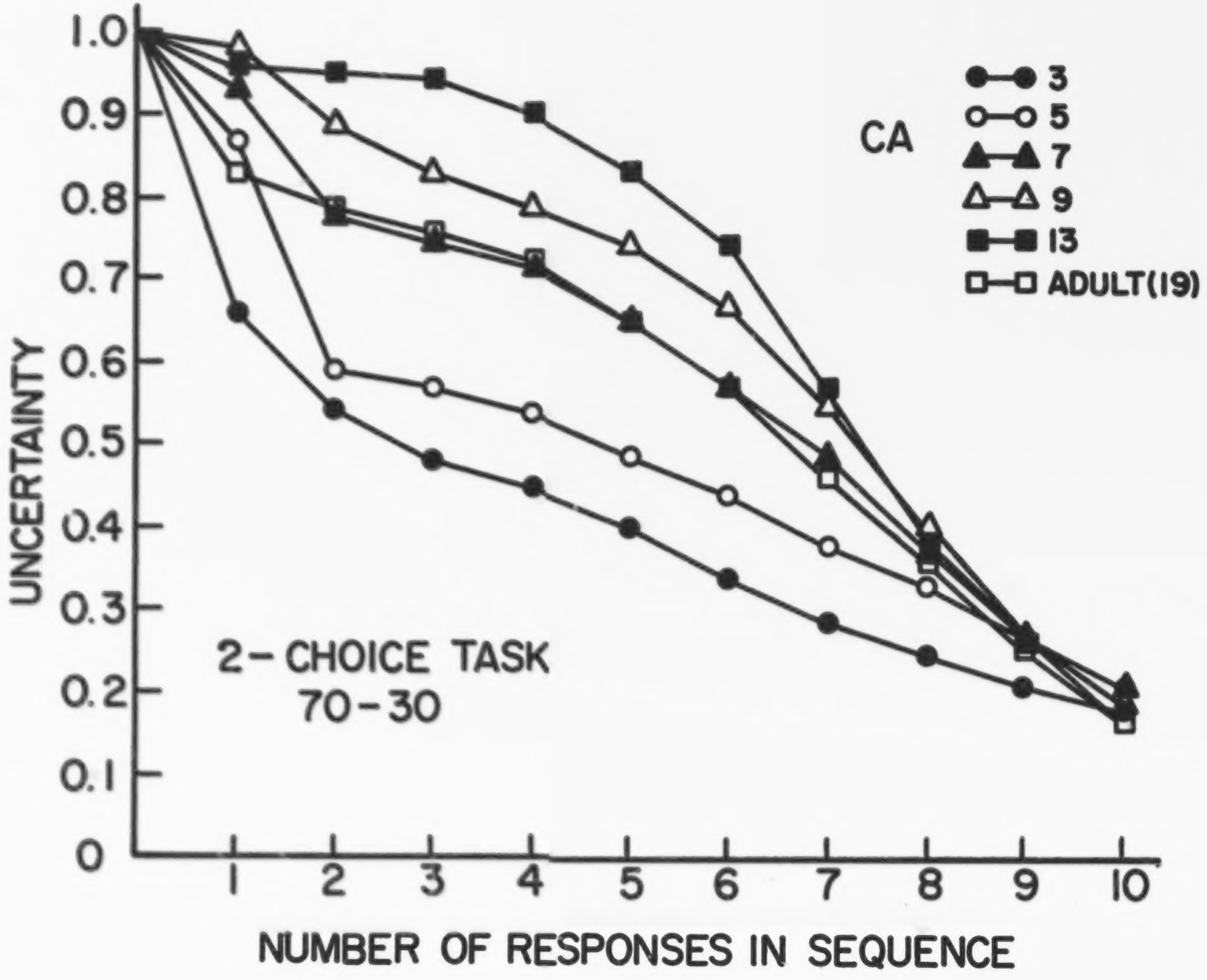

UNCERTAINTY
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
NUMBER OF RESPONSES IN SEQUENCE
1
2
3
4
5
6
7
8
9
10
2-CHOICE TASK
70-30
CA
●—● 3
○—○ 5
▲—▲ 7
△—△ 9
■—■ 13
□—□ ADULT(19)

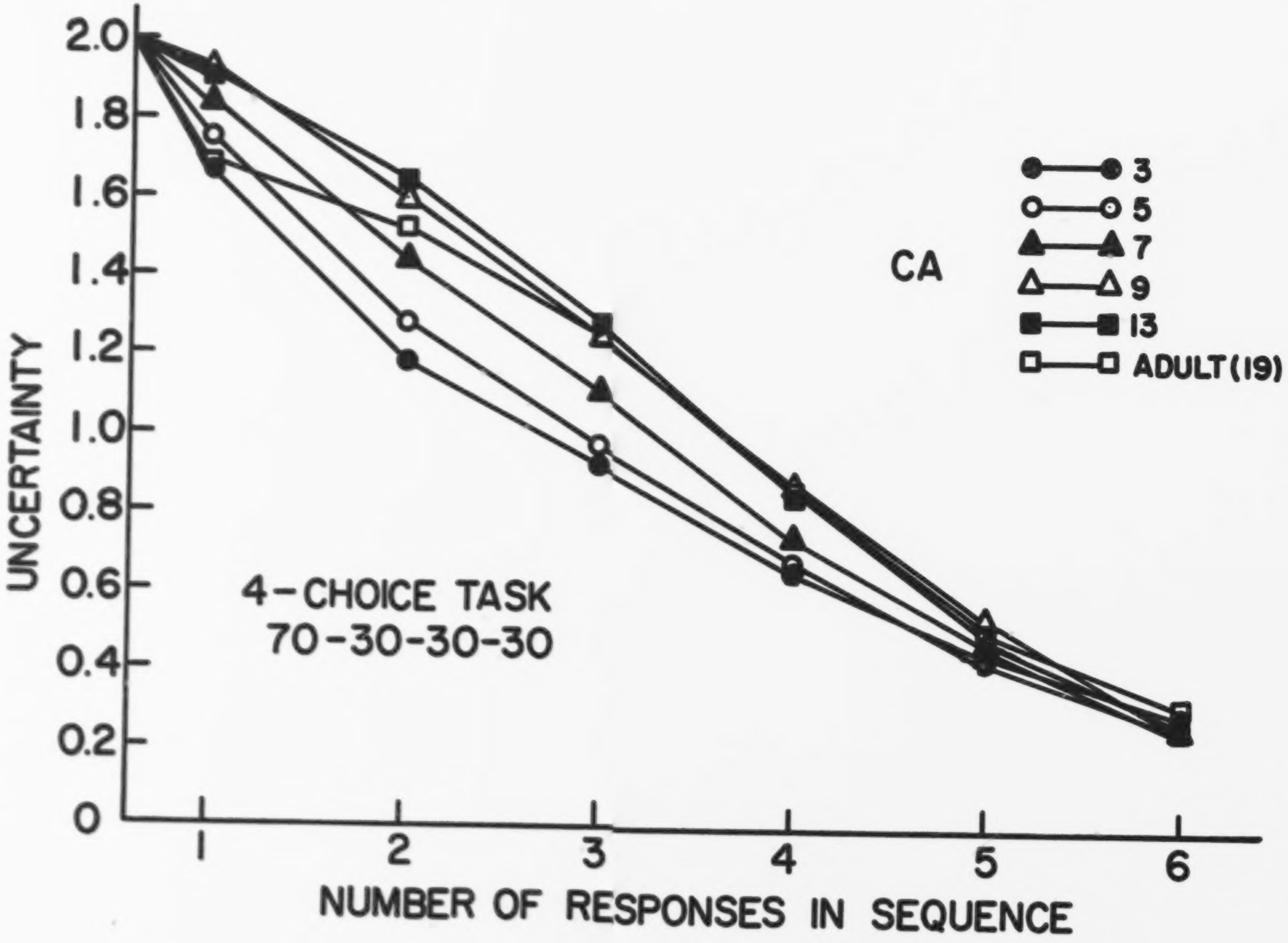
FIG. 8

six age groups. The result is a perfect developmental ordering except for the adult data. In order to clarify the meaning of this figure, it should be pointed out that the sooner a curve begins its downward deflection (e.g., the curve of the four-year-olds), the simpler are the response patterns being used repeatedly by the subjects. The further a curve progresses along the abscissa before making its major downward deflection (e.g., the curve representing the performance of the 13-year-olds), the more complex is the response sequenc In other words, this figure simply indicates the increasing complexity of responding in this task as age increases.

Figure 9 presents the same type of data for the four-choice, 70:30:30:30 task. Note that the abscissa includes only run lengths of up to six, as that is the limit of the memory of the computer used to analyze these four-choice data. Again, the developmental ordering is perfect, except for the adults.

When my colleague, Harry Munsinger, saw these two graphs, he suggested another way to examine these data. The idea is as follows: Assume there is a limit to a child's information processing capability, and, in both the two- and four-choice tasks, he works close to this processing limit. If it were possible to somehow equate these two tasks for difficulty level, the uncertainty reduction curves should be identical. We attempted to do this in the following manner: Each decision in the four-alternative task is, in informational terms, a two-bit decision, while each one in the two-alternative task is a one-bit decision. Perhaps the comparison we want could come from examining the amout of informatio reduction in the two-choice task following every second response in the sequence. Thus, each point plotted on the abscissa would represent 2 one-bit decisions in the two alternative task, and this curve could then be plotted on the same set of axes as is the curve for the four-alternative task.

Figure 10 presents such curves for the six age groups studied. Note the disparity between the four-choice and the converted two-choice curves for the younger children; note also that the degree of disparity decreases systematically as age increases. The young child (e.g., four to six years old) appears to behave in a fairly complex fashion in this task if the task itself is complex. If the task is simpler, then his response



2.0
1.8
1.6
1.4
1.2
1.0
0.8
0.6
0.4
0.2
0
UNCERTAINTY
1
2
3
4
5
6
NUMBER OF RESPONSES IN SEQUENCE
4-CHOICE TASK
70-30-30-30
CA
3
5
7
9
13
ADULT(19)

FIG. 9

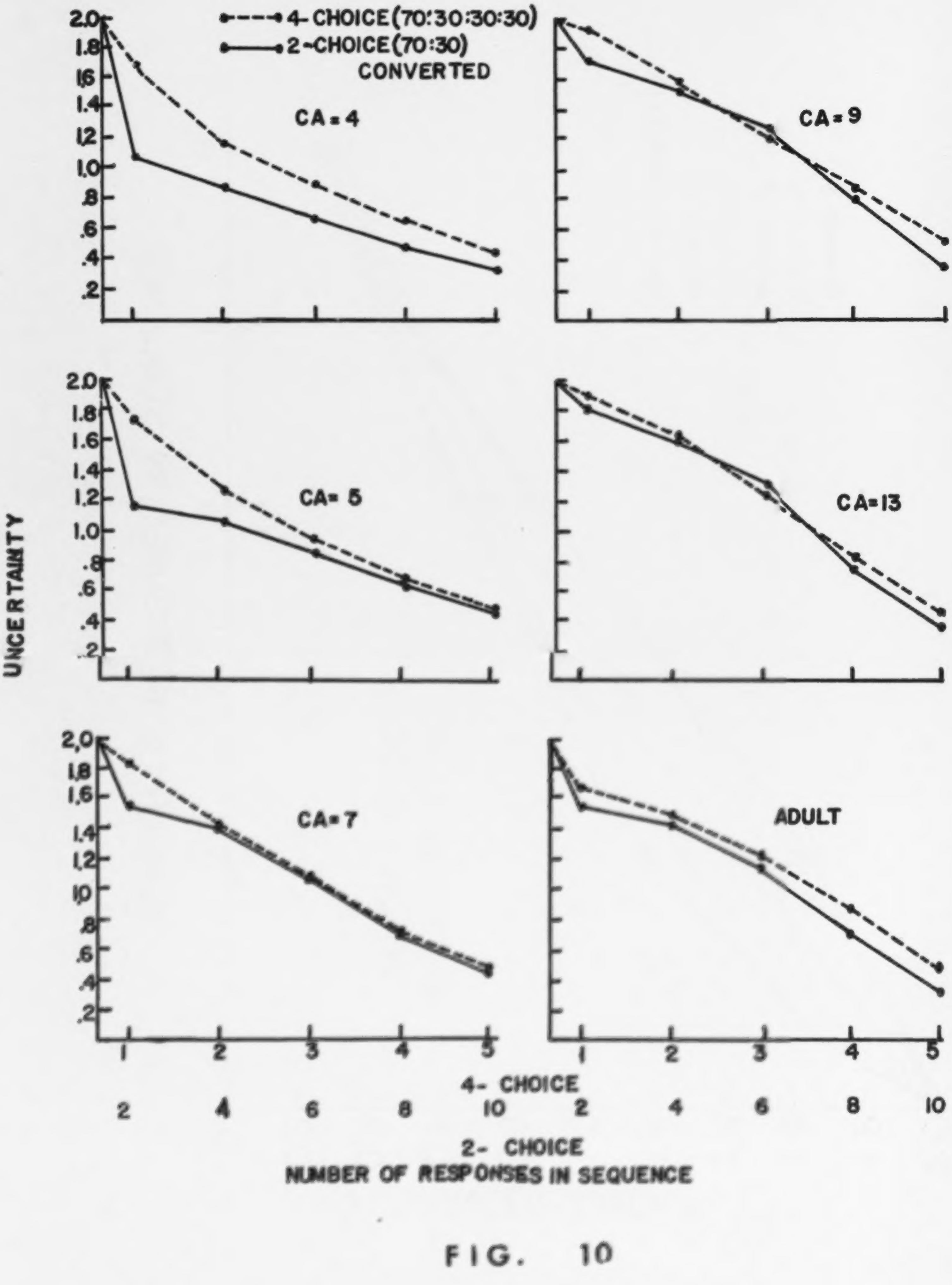

4-CHOICE (70:30:30:30)
2-CHOICE (70:30) CONVERTED
CA = 4
CA = 9
CA = 5
CA = 13
CA = 7
ADULT
UNCERTAINTY
2.0
1.8
1.6
1.4
1.2
1.0
.8
.6
.4
.2
1
2
3
4
5
4- CHOICE
2
4
6
8
10
2- CHOICE
NUMBER OF RESPONSES IN SEQUENCE

FIG. 10

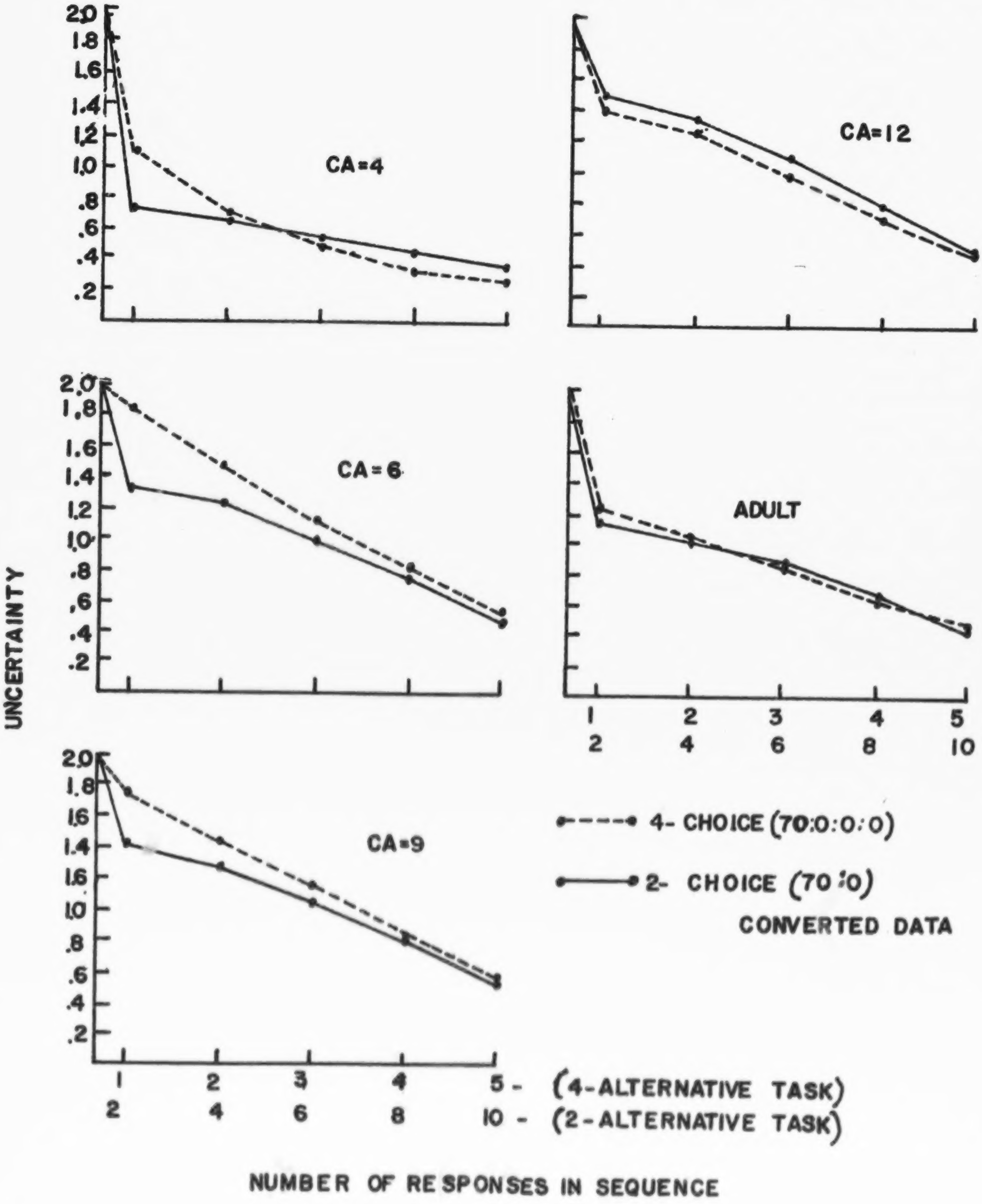

UNCERTAINTY
2.0
1.8
1.6
1.4
1.2
1.0
.8
.6
.4
.2
CA=4
CA=12
CA=6
ADULT
CA=9
1 2 3 4 5
2 4 6 8 10
4- CHOICE (70:0:0:0)
2- CHOICE (70:0)
CONVERTED DATA
5 - (4-ALTERNATIVE TASK)
10 - (2-ALTERNATIVE TASK)
NUMBER OF RESPONSES IN SEQUENCE

FIG. 11

sequencing is also simpler. This does not appear to be true of the older child--in terms of information processing, and the conversion used here, he behaves the same regardless of the complexity of the task, at least within the limits of complexity represented by this experiment. I have little doubt that the older child would show differences in informatio processing as a function of task complexity if the complexity were greater than it was in these studies.

Thus far I have not mentioned the 70:0 and 70:0:0:0 tasks. The converted curves for the 70:0 schedule are presented in Fig. 11 along with the data from the 70:0:0:0 condition. With the exception of the four-year-olds, this picture is about the same as that presented in Fig. 10. What is the explanation of the performance of these youngest children when compared with their performance shown in Fig. 10? Two possibilities have occurred to us. First, the data plotted in Fig. 11 for the four-year-olds represents only about half as many subjects as the rest of the curves in Figs. 10 and 11. The discrepancy may then be a sampling error. Second, in a 70:0 or a 70:0:0:0 task, children this young tend to maximi their choices of the pay-off alternative very early in the task. The same thing is true of children this age in a three-choice, 70:0:0 task. It may simply be that these children, lacking patterned strategies, are drawn to the only button that pays off solely on the basi of the absence of pay-off on the other alternatives and the large discrepancy between the percentage of pay-off between the reinforced alternative and the other alternatives. The result of this may be to destroy most of the effect that variation in task complexity might have for these children.

In general, it appears that the developmental ordering of these curves in consistent with the notion that the older child and adult can manufacture a complex set of responses, even when presented with a relatively simple task. The younger child, on the other hand, does not appear to be able to do so, and greater response complexity is seen to be associat with greater task complexity. In deference to the title of my presentation, which mentions children's memory, I am suggesting that one reason a young child's performance varies as a function of task complexity is that in order to produce complex response sequences in a relatively simple task, the child, not having complexity represented in front of him in the stimulus situation, must rely to a greater degree upon his own recall for information concerning responses which he has just made. Since the young child is less well able to do this than is an older child, his response patterning is less complex in a two-choice than in a four-choice task. The older child, on the other hand can remember, in the two-choice task, several preceding responses and their outcomes. Using this recall, he is able to manufacture as complex a string of responses as he is when greater complexity is actually coded for him in the task. Perhaps what I am saying is simply that the younger child is more "stimulus bound" than is the older child, and can produce complex response patterns only when the complexity is coded for him in the stimulus situation.

Reference

Weir, M. W. Developmental changes in problem solving strategies. Psychol. Rev., 1964, 71, 473-490.

Footnote

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Discussion and Comments: Memory Functions

Summarized by Edwin Martin

Since Ebbinghaus, the almost universal approach to memory has been to present something to an organism at time t_0 and later, at time t_1, to ask him to recall it. But if the organism is successful at t_1, we are left in doubt as to whether he in fact remembered it in toto or he simply stored information about critical aspects of it and from this information generated a correct recall. Conversely, if the organism fails at t_1, is it because he forgot part or all or what?

A good deal more insight about memory comes from experimental situations where the organism actively makes use of stored information in one way or another in on-going behavior. In tracking tasks, as in Bogartz's paper, performance relies heavily on the detection, storage, and retrieval of information about sequential dependencies. Bogartz made use of a very simple sequence of targets (regular alteration of two colors) and constructed a model of how he thought 4- and 5-year-old children might go about predicting which target was about to appear. The model takes into account the possibility of utilizing as the relevant information for prediction either an event trace or a response trace. As it turns out, the sequence of correct and erroneous predictions given by the children are very much like a Markov chain and Bogartz is therefore able to explain nicely how the processes of attention to either event or response traces operate.

Problem solving is another task situation that involves detection, storage, and retrieval of information. In a probabalistic situation, Weir studied the behavior of 3- to 19-year-olds in their attempts to get as many marbles as possible in a no-solution, but maximizeable, problem. The question of interest is, "What information is utilized by people in such situations?" That is, "What is it about the events gone by that people remember and use in subsequent behaving?" Weir's procedures allow for the separation of frequency and sequence-pattern hypotheses. Furthermore, his manipulation of the age variable permits first insights into the interaction of strategy-utilization and information-processing capabilities at different developmental levels. I think the most interesting outcome of this research is the idea that even when frequency information gives incontrovertible evidence as to what response is the only one reinforced, children cling tenaciously to a sequencing hypothesis of some sort. In other words, while *what* happened may be routinely stored in memory, there is nevertheless a persistent scanning of what happened *in what order*. Weir has argued that, developmentally, storage capabilities for event sequences may not develop at the same rate as certain tendencies toward response-sequence patterns (strategies).

Both Bogartz and Weir have addressed themselves to memory for sequential information, Bogartz by design, Weir by conclusion. These are basic data for an eventual explication of the role of memory in such sequentially structured behavior as language behavior.

Since the emphasis has been on memory for sequential information, I might mention certain data from our own laboratory. The task was to recall strings of words of varying lengths, but where the strings were either structured or not structured according to a well known set of rules, namely, English syntax. Let N stand for noun and V for verb. There were three different types of strings: All Ns, all Vs, and NVN triads. The Ns were common names, the Vs common verbs. Typical NVN triads were: *Ed pinches Barbara*, *Larry chases Fred*, and *Nancy knows Pat*. Using 4- and 5-year-old pre-schoolers, 30 in all, and 38 college sophomores, memory spans for these three types of materials were determined.

In Table 1 are shown the proportion recalls that were completely correct for varying numbers of units, Ns, Vs, NVNs.

Table 1

Proportion Completely Correct Recalls

Unit Type	Age Group	Number of Units							
		1	2	3	4	5	6	7	8
N	Adult	---	---	---	.98	.85	.46	.22	.00
	Child	---	1.00	.94	.42	.06	---	---	---
V	Adult	---	---	---	.96	.68	.44	.10	.04
	Child	---	.99	.91	.43	.14	---	---	---
NVN	Adult	---	.90	.12	.00	---	---	---	---
	Child	.99	.28	.00	---	---	---	---	---

If one converts these proportions to standard z scores and fits a straight line for each combination of unit type and age group, the spans shown in Table 2 are obtained. The spans in parentheses for NVNs are the ordinary spans for the NVN triads multiplied by 3.

Table 2

Spans and Span Reciprocals

Unit Type	Age Group	Span	Reciprocal of Span
N	Adult	6.1	.16
	Child	4.0	.25
V	Adult	5.8	.17
	Child	4.0	.25
NVN	Adult	2.5(7.5)	.40(.13)
	Child	1.8(5.4)	.56(.19)

Several relatively simple statements can be made. First, the spans for all three unit types are longer for adults than for children. Since this is true for Ns and Vs, as well as Ns and Vs in grammatical combinations, a clear increase with age in information-processing capacity is indicated. Second, the increase in span with age is not uniform over the three unit types. For Ns, the increase is 53%; for Vs, 45%; and for Ns and Vs in NVN combinations, where the structure of English is the rule for each combination, only 39%. Thus the gain over age is least when the string is structured. Third, note for adults and children that when Ns and Vs are structured into NVN triads, the spans are 7.5 and 5.4 units, respectively. This means for adults, the increase in number of units processable in immediate memory attributable to sequential structure is about 26%; for children, however, the gain is about 35%.

In brief, the span data show that (1) in passing from age 4-5 to college age, the increase in "raw" information-processing capacity is greater than any increase due to greater facility with the structure of English, and (2) the children gain more by the introduction of grammatical structure than do the adults.

Also in Table 2 are entered the reciprocals of the spans just discussed. Of what use are these? Consider the adult spans and assume a finite capacity for immediate information processing. If this facility can handle 6.1 Ns, then each N utilizes

(1/6.1)100 = 16% of the total capacity. Similarly, each V utilizes 17%. This means that on an independence hypothesis, an NVN combination should utilize 2(16%) + 17% = 49% of the immediate capacity. The NVN data, however, indicate that an NVN combination actually utilizes only 40% of the capacity for adults. Thus for adults, the introduction of grammatical structure brings about an 18% saving. The corresponding calculation for children indicates a 25% saving of available processing capacity due to English structure.

The overall result seems to be that an increase in age from 4-5 years to approximately 20 years brings about (1) decrease in processing expense for independent units (Ns and Vs), but (2) also a decrease in processing gain from the structure of English.

ASSOCIATIONS AND VERBAL HABITS

Wednesday afternoon, October 20, 1965

Klaus F. Riegel (Chairman)

Since the time of Kent, Rosanoff, Woodrow and Lowell, psychologists have been interested in age differences in associations and verbal habits. More recently, David S. Palermo, in cooperation with James Jenkins and Wallace Russell, has become identified with this area of developmental psychology. Moreover, he has applied the word association norms collected at Minnesota to numerous studies on verbal learning of children. Edward A. Bilodeau has been less concerned with developmental psycholinguistics during the past. Recently he has contributed a unique methodology for the study of verbal learning and verbal behavior which he describes in his paper. Both Palermo and Bilodeau had been invited as consultants for our Study B: Developmental Studies in Semantics (Klaus F. Riegel and Edwin J. Martin) and for Study E: Developmental Studies in Recognition and Recall (Edwin J. Martin).

Free-Association Norms and Associative Structure

Edward A. Bilodeau [1]

A very good demonstration of the effects of cultural word habits on the kinds of correct recall and misrecall to be expected after training on a short list of words was made two years ago (Bilodeau, Fox, and Blick, 1963). The essence of the procedure involved the use of a stimulus prompt or cue during the recall period to assist in the retrieving process. The recall stimulus was a word associated in various degree to the word presented during training, but a word which had not been administered to *S* during training. The stimulus word used by *E* was chosen because of its particular properties of response evocation exhibited in tests of free association. The free-association test had been administered earlier, elsewhere, and to other people. Hence, the findings showed *cultural* characteristics of associative structures of words to be a means of predicting the probability of a correct retrieval or the probability of a particular misrecall.

A few examples from free-association data will make the above clear. My examples have been selected from Categories A, B, and C, classifications of associative structure which have considerable use in the design of experiments.

Category C

	S_1	R_1	R_2	R_{3-n}
	light	dark	bulb	bright, etc.
p		.57	.06	.37

Above, *dark* is the primary associate of *light* and its probability [$p(R_1)$] equals .57, *bulb* is the secondary associate of *light* with $p(R_2) = .06$, and *bright* and so on are the remaining associates in the response hierarchy. Their combined probability, which I call $p(R_{3-n})$, equals .37. If *S* were trained on *bulb* and tested for recall of *bulb* in the presence of *light*, we might expect that (a) *bulb* would appear more frequently than when *light* is not used, (b) when *bulb* is not recalled, *dark* might intrude in its place, and

(c) if not *dark* intruding, then *bright*, etc. would. A Cat. C word is characterized as a stimulus where the value of $p(R_1)$ is relatively high in association hierarchies.

Category B

In another case, as illustrated below, R_1 and R_{3-n} form less competition to recalling R_2 because the probability of R_2 is near maximum in the hierarchies.

	S_1	R_1	R_2	R_{3-n}
	ocean	water	sea	blue, etc.
p		.31	.30	.39

Here R_2 should be easier to recall than in the example of Cat. C. If R_2 is not recalled, then the intrusion is more likely to be an R_{3-n} and less likely to be an R_1 than for words from Cat. C. A Cat. B word in the norms is characterized as a stimulus word where $p(R_2)$ is strong, though still less than that of the primary.

Category A

For Cat. A words, illustrated below, the $p(R_2)$ and $p(R_1)$

	S_1	R_1	R_2	R_{3-n}
	cheese	cracker	milk	cake, etc.
p		.11	.10	.79

are low, while $p(R_{3-n})$ is high. In this case, R_2 should be recalled about as well as in C above, but R_{3-n} intrusions should predominate over primaries. A Cat. A word is characterized as a stimulus word where $p(R_{3-n})$ is near maximum value in the cultural norms.

The Russell-Jenkins (1954) free-association norms were analyzed by Bilodeau, Fox, and Blick (1963) as suggested by my examples of categories above. A strong relationship between the primaries and secondaries was quickly discovered. This relation (eta equals .65) can be seen after careful examination of the 100 dots in the lower part of Fig. 1, a most important law being found. When $p(R_1)$ is very low or very high, $p(R_2)$ is low, as it must be; when $p(R_1)$ is of moderate strength, $p(R_2)$ reaches its maximum values. The best chance

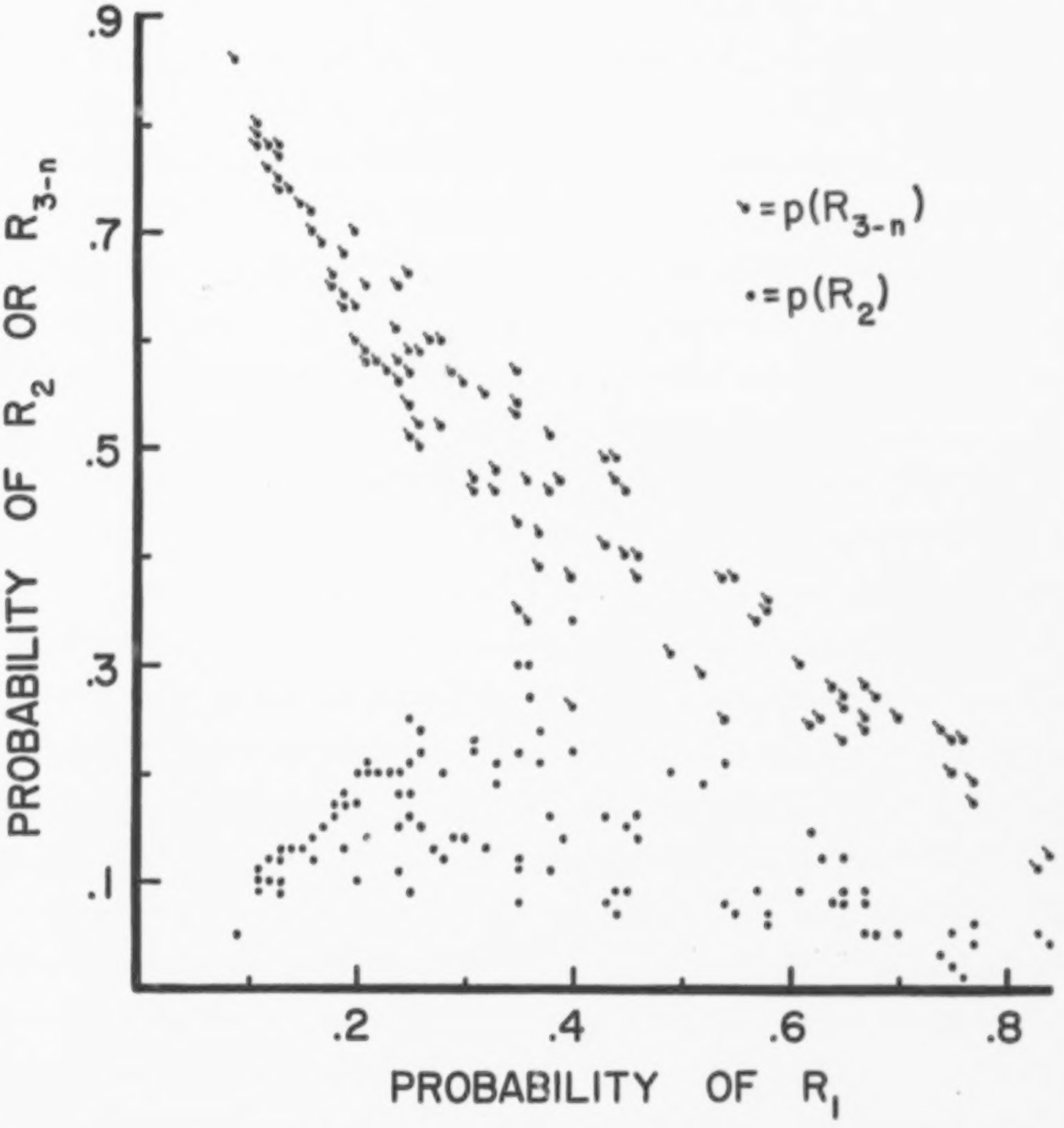

Fig. 1. Scatter plot showing the relationship between $p(R_2)$ or $p(R_{3-n})$ and the independent variable, $p(R_1)$ in the Russell-Jenkins hierarchies of associative responses. Note that $p(R_2)$ rises in magnitude and reaches a maximum when $p(R_1)$ is about .35 (after Bilodeau, Fox, & Blick, 1963).

of responding with R_2 is with $p(R_1)$ in the neighborhood of .35. A strong and necessarily negative relationship between $p(R_1)$ and the probability of responses 3 to n is shown by the 100 dots with tails in Fig. 1.

To summarize, three kinds of words or categories stand out in the Russell-Jenkins norms.

Category A: Low $p(R_1)$, low $p(R_2)$, and high $p(R_{3-n})$

Category B: Moderate $p(R_1)$, maximum $p(R_2)$, and moderate $p(R_{3-n})$

Category C: High $p(R_1)$, low $p(R_2)$, and low $p(R_{3-n})$

When used for stimulating recall, Cat. A words have maximized the incidence of responses 3 to n, Cat. B produced the most R_2s, and Cat. C produced the most R_1s.

I can give only three examples of recent research with categories from the Tulane laboratory. The first design varied Categories and Duration of Rest; after a brief rest, the recall scores resembled a recall process, but after a long rest, the process resembled free association (Bilodeau and Blick, 1965). In another study, primary words were put into the first list and secondary words were used in the second list (Blick, 1965). The primaries interfered enormously with the retrieval of the secondaries, the effect being accounted for by the strength of the primary in free-association norms. In the third study, the Ss were required to free associate as an activity interpolated between training and recall (Fox, 1964). As hypothesized, the individuals interpolated free associate interferes with recall or facilitates recall, depending on the normative properties of the stimulus. In these three studies then, and others coming along, we are gaining control over the individual subject and the word he utters. Of course, the use of norms is not restricted to learning and recall tasks but may be generalized to any word-communication task (Rosenberg and Cohen, 1964).

The need for associative norms has been growing rapidly as a glance at recent journals of experimental psychology will show. During the last decade, the Russell-Jenkins data (1954) have been frequently used. Similar norms by Palermo and Jenkins (1964) were published last year. Riegel just published norms on restricted association (1965). These norms specify responses and frequency of response to common stimulus words. They constitute an important source of raw data for investigators in psychometric and experimental traditions.

Another useful set of associative structures was published a few months ago by Bilodeau and Howell (1965). Newer, improved, and different associative probability structures are expected to be discovered in these norms and it is this book of norms which I wish to describe below. For discrete association, S responded only once to the stimulus word; for continued association, S responded three times to the stimulus word, the stimulus being administered three times.[2] The continued norms may yield greater accuracy in estimating response arousal through the probability with which a given response is found in the first three responses of Ss. A secondary response is not necessarily weak because its probability is low in Discrete association. It might have a considerable probability should S be given another opportunity to respond. A low probability for R_2 in discrete association does not necessarily mean that it does not compete strongly even at the time the primary is emitted.

The program for Discrete norms called for 324 Ss and that for Continued, 972 Ss. A small booklet presented the task. The stimuli were 180 in number, 45 being the Index words or Cat. A, B, and C words I described earlier, the remainder the primary, secondary, and tertiary responses--or the top of the hierarchy. Because the stimulus words were taken from the top of the hierarchies, the new norms will show how related words lead to each other and to words in common.

Tables for discrete association: Probability hierarchies for the 45 Index words in Discrete association were calculated, one sample being shown in the Appendix, page 48. Associates with $p = .01$ and not overlapping with other associates have been collected together and are shown on page 49 of the Appendix.

Index No. 2 on page 48, that is Whistle, Blow, Train, Noise, illustrates the reading of the table. Whistle arouses train ($p = .14$), blow, girl, etc. as well as loud (.06) and sound (.06). Blow, when used as a stimulus, arouses wind ($p = .56$), horn, hard, etc. The table shows how the analysis is arranged to feature hierarchical orders, overlapping associates, and high probability events such as $p(R_1)$. On page 49 of the Appendix the matrix at the bottom shows some of the p .01 words for Whistle (bait, ball, bird, ... yell).

This zero-one matrix shows that the R_1, R_2, and R_3 associates of Whistle do not evoke the $\underline{p} = .01$ words just mentioned. The table contains only diagonal matrices whose diagonal element is 1 (yes), e.g. an identity matrix. The associate in question is evoked only once and only by one of the four stimuli.

Should two or more stimuli evoke the same associate and $\underline{p} = .01$ for each, the phenomenon would have been seen on page 48. There were no such overlaps for Whistle though they do occur here and there for other Index words. Some $\underline{p} = .01$ events are entered on page 48 in the Whistle matrix, however. In all such cases, another stimulus has evoked the associate in question with $\underline{p}$ .01. By now it will have been seen that there are three kinds of uniqueness among associates, the type in the identity matrices (page 49) containing the largest number of elements.[3]

Finally, the Whistle matrix appears in the 2nd position among the 45 indices of the whole table (in the book) because the $\underline{p}(R_1)$ for Whistle has rank 2. Thus, the pages are not ordered arbitrarily but according to an important property of the matrix, the dominant event (Bilodeau, Fox, and Blick, 1963). In the case of Whistle, its primary is weak, it is a Category A word, and it arouses a relatively large number of different words.

Tables for continued association: On page 50 of the Appendix the behavior of Whistle in Continued association is shown. As with the Discrete norms, the associates with $\underline{p} = .01$ and not overlapping with others have been segregated and are collected elsewhere in order to organize these common elements of hierarchies. The most important difference between the Continued and Discrete hierarchy is that the three Continued associates (C_1, C_2, and C_3) are represented as columns. The words of an Index take up four consecutive pages.

Again I use Whistle as the Index and for an example in table reading. It arouses train, blow, noise, and so on, in the first opportunity; train, girl, tune, and so on, in the second opportunity; and girl, tune, stop, and so on, in the third opportunity. The zero-one matrix (100) at the top on page 51 contains the $\underline{p} = .01$ words of C_1 with no instances in common with C_2 and C_3. Matrices (010) and (001) represent those associates strictly unique to C_2 and C_3, respectively. Those associates which overlap appeared in the previous table as (111), (110), (101), or (011).

Tables for discrete and continued networks of association: The entries on page 52 of the Appendix contain information already presented in the earlier ones, the information being reorganized for better access. Here, the associative networks for Discrete and Continued association for the top four terms of the hierarchies are given. The *ps* for Discrete association are given under Column D, those for Continued under C_1, C_2, C_3.

Again Whistle, Blow, Train, Noise (Index No. 2 in the upper right) is used as my example.[4] S_0 = Whistle, R_1 = Blow, R_2 = Train, and R_3 = Noise, so that 01 means, using Whistle as a stimulus and Blow as a response, the *p* of Blow in Discrete association is .13; in the first Continued association it is .11, in the second it is .03, and in the third it is .04. The notation 10 means using Blow as a stimulus and Whistle as a response. The whole network from 01 to 32 is represented under Column SR (stimulus response).

To conclude, I have shown that the new norms contain processed, organized data still raw enough to provide new information on associative laws for those readers who will wish to process them further. In my own laboratory they are being used extensively in studies of verbal retention (Blick, 1965; Bilodeau and Blick, 1965; Fox and Bilodeau, 1966). My major purpose in presenting these norms here is to encourage further examination of the characteristics they contain and the experimental variation of these characteristics for word-communication studies.

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Footnotes

1. Currently on Leave of Absence granted by the Tulane University Council on Research and serving as Visiting Professor at the University of Washington. This paper reviews some of the research performed under Contract Nonr-495(10) between the Office of Naval Research and Tulane University.

2. A comparison of the methods was made earlier by Rosen and Russell (1957), and by Cofer (1958).

3. At the foot of the matrices on the page 48 are found (a) the number of words with $\underline{p}$ = .01 entered in the column above it; (b) the total number of words with $\underline{p}$ = .01, a figure obtained by summing the corresponding values from pages 48 and 49; (c) the total number of different words (W) evoked by the stimulus; and (d) the N or total number of subjects responding with a word.

4. Whistle in Discrete association arouses blow ($\underline{p}$ = .13), train ($\underline{p}$ = .14), and noise ($\underline{p}$ = .06). In Continued association, the corresponding $\underline{p}$s are .11, .17, .10 the first time; .03, .17, .01 the second time; and .04, .03, .04 the third time.

Appendix

Sample Pages of Probability Hierarchies in Discrete and Continued Association

2. Whistle, Blow, Train, Noise

	Whistle	Blow	Train	Noise
WHISTLE				
train	.14	-	-	-
blow	13	-	-	-
girl	11	-	-	-
noise	6	-	2	2
dog	4	-	-	-
call, happy, music	3	-	-	-
cop, lips, sing, siren, song, stop, work	2	-	-	-
BLOW				
wind	-	56	-	1
horn(s)	-	11	1	1
hard	-	6	-	-
nose	-	4	-	1
suck	-	4	-	-
breeze	-	3	-	-
up	-	2	-	-
TRAIN				
track(s)	-	-	12	-
car(s)	-	-	10	1
whistle	-	1	6	-
plane, travel	-	-	6	-
bus, caboose, smoke	-	-	5	-
engine	1	-	4	-
boat	-	-	4	-
fast, go, railroad	-	-	3	-
choo choo, locomotive, ride, transportation, truck	-	-	2	-
NOISE				
loud	6	-	-	23
sound(s)	6	-	-	14
quiet	-	-	-	8
racket	-	-	-	5
silence	-	-	-	4
ear, gun, quite	-	-	-	3
crash, din, radio	-	-	-	2
Prob. blanks	.01	.01	.01	-
No. .01 words above	1	1	1	4
Total No. .01 words	21	14	19	33
No. of words (W)	38	21	38	45
No. of responses (N)	107	107	107	108

1. Yellow, Color, Green, Blue

(1000) balloon, beautiful, book, canary, Chinese, dingy, dress, frighten, hair, jaundice, lemon, pale, ribbon, sign, teeth, three, tiger

(0100) beauty, blonde, pictures, pretty, race, see, skin, spectrum

(0010) bay, clover, giant, go, horn, land, lettuce, money, pastures, pea, people, seen, ugly

(0001) Air Force, Chris, clothes, coat, cool, day, flag, grey, jeans, mood, red and white, shade, suit, true

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2. Whistle, Blow, Train, Noise

(1000) bait, ball, bird, cat, hum, hymn, noon, pig, police, shout, shrill, softly, swan, toot, toy, tune, wolf, women, wow, yell

(0100) balloon, beneath, billows, blew, breath, cold, exhale, fist, hit, Joe, puff, still, under

(0010) B & O, black, black noisy, brain, diesel, long, motion, move, noisy, rail, rain, robbery, speed, station, strong, toy, transport, wheel

(0001) band, bang, bar, barracks, bathroom, bay, big, chaos, commotion, deaf, distrubance, drum, eyes, face, frighten, generator, headache, hear, heat, load, lockers, maker, morning, mouth, pain, person, shut up, silent, smell

For additional prob. .01 words see page 48.

②

5a. WHISTLE, Blow, Train, Noise

	C_1	C_2	C_3
train	.17	.17	.03
blow	11	3	4
noise	10	1	4
loud	4	3	2
sing	4	2	2
work	3	2	1
song	2	2	1
shrill, siren	2	1	1
talk, tweet	2	-	-
girl(s)	6	9	7
tune	7	8	6
sound	3	4	1
horn	2	3	-
music	1	3	2
cop	-	3	-
toot	1	2	-
policeman, ship	-	2	1
hear	-	2	-
stop	3	3	5
happy	3	1	5
dog	1	3	4
bird(s)	1	-	4
boat	-	1	4
call, steam, wolf	1	1	2
fire	1	-	2
wind	-	1	2
police, shriek	-	-	2
lips, note	1	1	-
mouth	1	-	1
clean, fire drill, warning	-	1	1
Prob. blanks	.04	.05	.10
No. .01 words above	11	14	11
Total No. .01 words	15	26	29
No. of words (W)	32	44	49
No. of responses (N)	101	100	95

5a. WHISTLE, Blow, Train, Noise

WHISTLE

(100) at, bell, shout, soda

(010) black, car, fall in, for, fun, hum, long, lunch, medium, person, so, yell

(001) attract, ball, boy, cat call, drill, fire engine, go, Lackland, pipe, retreat, shiny, signal, snore, soft, steamboat, teeth, to, wow

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5b. Whistle, BLOW, Train, Noise

BLOW

(100) blast, explode, fight, hot, slow, train, windy

(010) belt, blower, bomb, claringet, draw in, fast glass, grow, jump, loud, lungs, sail, sky, still, threw, toot, tornado, under

(001) bag, beneath, blare, blown, boy, bugle, clouds, dryer, dust, easy, exhaust, faster, French horn, hair, heart, in, mature, news, noise, over, paint, pass, people, pipe, remote, sailboat, shell, ship, smoke, snow, wint

1. Yellow, 2. Whistle, 3. Head, 4. Cheese

S_0	Yellow
R_1	Color
R_2	Green
R_3	Blue

SR	D	C_1	C_2	C_3
01	.09	.16	.07	.06
02	09	06	08	05
03	06	06	02	06
10	03	01	02	03
12	06	04	07	09
13	26	21	14	16
20	06	07	05	02
21	04	10	10	02
23	11	11	05	02
30	00	00	02	02
31	10	17	05	05
32	04	07	03	02

S_0	Whistle
R_1	Blow
R_2	Train
R_3	Noise

SR	D	C_1	C_2	C_3
01	.13	.11	.03	.04
02	14	17	17	03
03	06	10	01	04
10	01	02	02	00
12	00	01	00	00
13	00	00	00	01
20	06	06	06	06
21	00	00	00	00
23	02	04	04	04
30	00	00	00	00
31	00	00	00	00
32	00	02	00	01

S_0	Head
R_1	Foot
R_2	Hair
R_3	Shoulder

SR	D	C_1	C_2	C_3
01	.06	.06	.02	.01
02	13	05	14	07
03	11	06	05	00
10	01	00	00	00
12	00	00	00	00
13	00	00	00	00
20	16	11	02	02
21	00	01	00	00
23	00	00	00	00
30	05	06	01	04
31	00	00	00	00
32	00	00	01	00

S_0	Cheese
R_1	Cracker
R_2	Milk
R_3	Cake

SR	D	C_1	C_2	C_3
01	.06	.01	.06	.02
02	08	14	07	08
03	15	14	03	02
10	06	03	04	06
12	03	01	03	00
13	00	00	00	01
20	00	01	00	00
21	00	00	00	00
23	00	00	00	00
30	00	00	00	00
31	00	00	00	00
32	02	00	00	01

Word Associations as Related to Children's Verbal Habits[1]

David S. Palermo

In 1964 Jenkins and I published a set of free association norms for 200 words based upon the written responses of 250 boys and 250 girls in each of the grades 4-8, 10 and 12, and 500 males and 500 females in college (Palermo and Jenkins, 1964). We needed the norms for two purposes: (1) to determine whether the associative habits of children are different from those of adults, and (2) to attempt to evaluate the influences which such differences, if found, might have on the verbal learning and verbal behavior of children.

In order to evaluate the differences between child and adult free association responses we examined some of the frequency, grammatical and semantic characteristics of the responses given by the various grade groups. These analyses of the normative data have revealed a number of clear developmental trends. First, the average frequency of the most popular responses increases from Grade 4 through College and the average number of different responses given to any stimulus decreases over the same age span. Not only are the frequency characteristics different but, of the actual words used as responses, only about 50% of the most popular responses given by fourth grade children are the same as the most popular responses given by college students (Palermo, 1963). Examination of semantic relationships between the stimulus and response words reveals that the frequency of responses which are superordinates relative to their stimuli increases from Grade 4 to Grade 6 and declines steadily from sixth grade to college (Palermo and Jenkins, 1963). Contrast or opposite responses, on the other hand, steadily increase in frequency from Grade 4 through College (Palermo, 1964). Finally, analysis of the grammatical relationships between the stimuli and responses indicates that the frequency of paradigmatic responses increases from Grade 4 through College although the initial level and rates of increase vary considerably from one grammatical class to another. In addition, for all of these developmental trends, the typical finding that girls are more advanced than boys in various language measures is consistently apparent (Palermo, 1963). While there are many kinds of analyses which could be made of the normative data, these results appear sufficient to clearly establish the fact that the associative habits of children are different, at least in degree, from those of adults. Furthermore, a comparison of the normative data of the fourth and fifth grade children in our sample with those of a comparable group of fourth and fifth grade children collected by Woodrow and Lowell in 1916 (Palermo and Jenkins, 1965) indicates that there are marked changes which have occurred over the 45 year period between the two norm collections. All of the changes suggest that children today are more like adults in their responses. I have argued that this is one indication that children are more linguistically sophisticated now than was the case 45 years ago, although the one attempt we have made to relate language development to word association responses was not particularly encouraging (Tobiessen, 1964).

Since marked differences in the associative characteristics of the responses of children and adults have been demonstrated, the primary focus of the research has turned to the influence which such differences might have on the performance of children in a variety of verbal learning and verbal behavior tasks. Thus far, we have examined the influence of verbal associative habits upon paired-associate learning, mediation, recall, tachistoscopic recognition, generalization and discrimination. Reports on some of these data have been published, so I will only review the procedures and results briefly.

The research began with an examination of the influence of normatively reflected associative habits upon the paired-associate learning of children. Using both mixed and unmixed list designs, it has been demonstrated (Wicklund, Palermo, and Jenkins, 1964) that differences in associative strength do influence the rate of paired-associate learning of children. The differences in rate of learning are clear regardless of whether comparison is made of the rate of learning responses at various positions in the associative hierarchy of the same stimulus or, whether comparison is made of the rate of learning pairs composed of stimuli and their primary responses where associative strength of the primary varies. The stronger the associative strength between the stimulus and response the faster the rate of learning. The most interesting aspect of these findings is the fact that they cannot be replicated with college students. Unless low frequency words are used (Postman, 1962; Martin, 1963), college students learn a list of words which are associated to any degree so rapidly that no differences in learning rate may be demonstrated except between some level of associative strength and none. The fact that associative strength affects the learning rate of children but not that of adults may be interpreted as indicating that the associative connections between words, reflected in the norms, are positively related to age, presumably reflecting differences in language experience.

In order to extend these findings and to attempt to determine when the associative strength variable loses its effectiveness, Wicklund (1964) has replicated the earlier studies with children in grades 4, 6, 8 and 10. The results of this study, rather surprisingly, showed that the associative strength variable continues to affect rate of learning in a statistically significant manner even at the 10th grade level. At all grade levels, Wicklund found that pairs composed of stimuli and their primary responses of 20-50% normative strength were learned faster than pairs with 10% normative strength which in turn were learned faster than pairs with a 1% normative strength. All groups learned in less than half the trials necessary for a group to learn a list of unassociated word pairs. Thus, the differential influence of associative strength on paired-associative learning does not disappear until after the 10th grade but before college. The possible alternative interpretation that there is a difference in the populations sampled, does not obviate the relationships observed, but might be worthy of investigation to clarify the effects of language facility on the associative strength variable.

A second type of task in which the influence of the associative strength variable has been examined is that of clustering in recall. On the basis of three experiments, conducted with fourth and sixth grade children, there is ample evidence to conclude that associative clustering does occur in the free recall of children of this age. These studies have shown, with fourth grade children, that given a randomly arranged list of 30 words within which are 15 pairs of stimuli and their primary responses taken from the appropriate norms, recall of the list will occur in associatively related clusters of stimulus and response pairs. Furthermore, the probability of clustering is positively related to the associative strength of the stimulus and the primary response, at least in the range from 10 to 64% (Wicklund, Palermo, and Jenkins, 1965). As might be expected, the total amount of recall is less for children than for adults, but more significantly, the relative percentage of clustering as a function of opportunities to cluster appears to be less for children than for adults. In an unpublished study with sixth grade children (MacLean, 1963), it was found that the amount of clustering could be increased by repetition of the list three or five times. The amount of repetition necessary to influence clustering was less for a list composed of pairs with an average associative strength of 56% than for a list of pairs with 14% strength. The data of these three studies further support the contention that the associative strength reflected in the normative data of children does not have as strong an influence upon the behavior of children as is the case with adults where amount of clustering has been shown to account for a larger proportion of the variance in this situation (Jenkins, Mink and Russell, 1958).

A third type of task investigated involves the Mink (1963) associative generalization procedure. Here the subjects, fifth grade children, were presented a list of 12 stimulus words from the word association list and told to try to remember the words and to press a telegraph key as each word was presented. After two presentations of the list, a second list of words was presented with instructions to press the telegraph key each time a word from the first list appeared. The second list was composed of six of the first list words, the six primary associates to those words, the other six words of the first list and six control words which had no associative strength to any of the 12 words in the first list. The associated words had a mean normative strength of approximately 64%. The percent of presses was approximately 84% to the first list words regardless of whether their associates were in the list or not, 23% to the associated words and 7% to the control words. A second study, with some minor modifications, replicated the results of this study. In contrast to the studies of paired-associate learning and clustering in recall, the results of the studies of associative generalization provide little evidence for relative differences in performance of children and adults. The amount of generalization to the associated words, and the errors to the control words are approximately the same for the children as in the studies conducted by Mink (1963) and Martin (1961) with college students.

Still a fourth type of task in which the influence of associative connections has been observed in children was reported in a recent thesis by Gallagher (1965) in our laboratory. Using stimulus words from the word association list as fixation words, he determined the recognition thresholds of words associatively related to the fixation word. His subjects were drawn from the fourth, eighth and twelfth grades. The results indicated that recognition thresholds decrease from fourth to twelfth grades,but, most importantly, at all grade levels the words associatively related to the fixation word were reported accurately at shorter exposure intervals than words with no associative relationship to the fixation word. While this relationship appears to be primarily due to the establishment of an associative guessing set created during the task (Verinis and Cofer, 1964), the results indicate that such sets can be as easily established in the fourth grade child as has been demonstrated for the twelfth grade child and for college students (O'Neil, 1953).

Verbal mediation in the paired-associate situation is a fifth area toward which attention has been directed. Thus far, we have used four of the eight three stage mediation paradigms (Jenkins, 1963): two chaining paradigms (Nikkel and Palermo, 1965; Flamer, 1964), a reverse chaining paradigm and a response equivalence paradigm (Palermo, in press). In each case, the first link in the associative chain was assumed from the normative data for sixth grade children, and the children were required to learn the second and third stages using trigrams as the third member of the associative chain. In all cases, using both mixed and unmixed lists, comparisons have been made of performance on lists arranged to produce mediated facilitation and interference relative to performance on control pairs for which no known mediational links were present. Although the magnitude of the differences between facilitation and control and interference and control conditions has varied with the paradigm and the use of mixed and unmixed lists, the results have consistently provided support for the mediation hypothesis. The results of these studies, as was the case with associative generalization, appear to be as definitive as any which have been obtained with adults. As a matter of fact, it would appear that the influence of the mediational effects last well into the learning task whereas the studies with adults find mediation effects are most prominent in the early trials.

One other study should be mentioned here in contention with both the early studies of paired-associate learning as a function of associative strength and the studies of mediation. Taking a lead from a theoretical model proposed by Jarrett and Schiebe (1963), children were used to test the hypothesis that not only the rate of learning direct associates but also the rate of learning pairs of words which are associated only through a mediating word would be a function of associative strength (Palermo and Jenkins, 1964). Strong support was found for Jarrett and Schiebe's model, which proposed that multiplying the associative probabilities of two pairs of words A-B and B-C would allow prediction of the rate of learning a pair, A-C, which has no direct associative connection. The data suggest that a list of pairs of words, A-B, which are directly associated with a probability according to the norms of, say, .20 will be learned at the same rate as a list of pairs of words, A-C, which are not directly related but do have the connection that A elicits B with a probability of .50 and B elicits C with a probability of .40. Thus, with children, it is possible to predict the relative rate of learning directly associated pairs, as indicated previously, and, in addition, we can account for the rate of learning pairs in a mediation situation based on multiplying the associative probabilities of the individual links in the mediational chain. Again, the difficulty of demonstrating such relationships with college sophomores might be emphasized because of the apparently high degree of association which any word which appears in the adult norms seems to have in terms of influencing paired-associate learning.

Finally, a partial failure in the experimental program should be presented. Since it had been demonstrated that children generalize from one word to its associate in the Mink associative generalization situation, it was assumed that given a situation in which discrimination between words is required, children would have more difficulty discriminating between associatively than non-associatively related words. Furthermore, the amount of difficulty in the discrimination task should be positively related to the strength of the associative connection between the two words. In an initial effort to test this hypothesis, two lists of 10 word pairs were constructed; one of high strength primary associates and the other of low strength primary associates. Fifth grade children were randomly assigned to a list and required to learn the 10 simultaneous discrimination problems in each list as they were presented on a memory drum. While the children did not make a large number of errors, there was no indication that they were having more difficulty with the strongly associated pairs than the others.

Since that time, four other experiments have been completed on discrimination learning with college students using simultaneous and two forms of successive discrimination learning procedures. The results demonstrate convincingly that our original hypothesis was correct, at least for the older population. In the three studies which have been analyzed, it has been found consistently that pairs of words which are highly associated prove to be significantly more difficult to discriminate than pairs which are not associated according to the normative data. Pairs of words with an intermediate level of associative strength have fallen in between, although performance has not always been statistically different from both the high and the unassociated word pairs. Analysis of the difficulties with the study conducted with the children leads to the conclusion that either the task was not difficult enough to demonstrate differences or that associative strength is not a strong enough variable to affect the performance of grade school children in this type of task. One of my students is in the process of setting up a study to clarify which of these hypotheses may be correct.

By way of summarizing the data presented thus far, it appears that associative data collected from children not only show characteristics which are different from those of adults but, in addition, the influence of these associative characteristics on the performance of children in a variety of verbal learning tasks reveals a number of differences. The influence of associative strength on paired-associate learning is clear, and performance in this task is quite sensitive to variations in associative strength, both direct and mediated. The amount of clustering in free recall is apparent but not so marked as is the case for adults. The perceptual recognition, associative generalization and mediated generalization tasks have not shown clear differences relative to the findings obtained with the college sophomore, although in the latter task, the effects of associative strength appear to influence the performance of children throughout the learning task rather than being limited to the early trials. At this point, little can be said about the relative performance of children and adults on a verbal discrimination task.

The experimental problems discussed up to this point have dealt largely with upper elementary school children. While clear differences have been established between the performance of these children and the college sophomore, the results have continually suggested that the use of younger children might allow further insights into some of the issues involved. Thus, normative word association data have been collected from children in grades 1-4 for 100 of the 200 words used in the original normative collection. The tests were administered orally and responses given orally by 50 boys and 50 girls in each of the grades. The stimulus words included all the parts of speech as well as superordinate and contrast-eliciting stimuli.

Analysis of these normative data indicates that all of the trends observed in the larger norms extend downward on the age dimension as might be anticipated. As with the other norms, the popular responses increase in frequency, as do superordinates, contrasts and paradigmatic responses. The most striking aspects of the data are the much larger grade to grade increases observed, particularly the changes from Grade 1 to Grade 2. One is tempted to be impressed by what the child apparently learns about his language in the first grade. It is perhaps worthy of note here that the oral administration has the effect of increasing the probability of a popular response by approximately 35%, increasing the probability of a contrast response approximately 100% and decreasing the probability of a superordinate response approximately 20%. Paradigmatic responses also are more probable under the oral administration procedure. These latter results are based upon a comparison of the fourth grade children who were given the oral administration and the original norm group which took a written form of the test.

Experimentally these normative data have been used for only one study at this date. Last year a study was conducted which was patterned after the earlier studies by Wicklund in which the influence of associative strength on paired-associate learning was examined. A tape recorded presentation of the lists was used with oral responses by first and fourth grade children. At each grade level, three different lists of eight pairs were constructed in which the associative strength of the stimuli and responses was approximately 20-45%, 5% or 0%. All subjects were run for a minimum of 15 trials. Significant differences were found in the rate of learning the lists in line with the previous findings using visual presentation with older children. The size of the performance differences between lists were much larger for the first grade children than for the fourth grade children. There were no inversions in the predicted order of the means on any trial for the first grade children while the fourth graders had reached nearly perfect performance on both of the associated lists by the fourth trial. The control, or 0%, lists were markedly more difficult in both grade groups. The data of the first grade are particularly encouraging for two reasons: first, the children adapted to the procedure of aural paired-associate learning beautifully and, second, the effects of the associative strength variable were obvious and lasted well into the learning task. It now appears quite feasible to attempt some studies of mediation with the younger children as well as some of the other kinds of tasks which have been used with the older children.

Finally, there is a serendipitous finding which has come out of this work which is both interesting and puzzling. In several studies in which control lists of unassociated words have been used, it has been found that when an unassociated word has been paired with a stimulus which has a strong primary response it is easier to learn that pair than if the word is paired with a stimulus which has a weak primary response according to the normative data. As of this date, this finding has been replicated six times. Thus far, the use of mixed or unmixed lists, part of speech of the stimulus, and age of subjects have been eliminated as variables which can account for these results. In the most recent study, trigrams were used as responses and the result was once again replicated. There is one hitch to the finding: exactly the opposite finding was obtained with one fourth grade group which learned a control list in Wicklund's doctoral dissertation. The question remains for our laboratory: Why is it easier (most of the time) to learn a list of unassociated words when the stimuli in the list have high strength primary free associates than when the stimuli have low strength free associates?

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Footnote

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Discussion and Comments: Associations and Verbal Habits

Summarized by Klaus F. Riegel

1. Both speakers, and in particular Dr. Bilodeau, emphasized the concept of associative structure. Indeed it would seem rather simple-minded to use association norms for nothing but the prediction of pair-wise connections between words, for instance, between the stimulus and response terms in paired-associates studies. Associative norms can provide much more information than this. Deese (1962) has analyzed the associative structure of free word-associations. I have made a similar attempt with restricted associations (Riegel and Riegel, 1963), and Rapaport (1965) has analyzed associative nets.

Of course, the question of associative structures is a rather general one and we could have devoted the entire meeting to a discussion of it. Even though I realized that no definite answers were available, I had raised the question in the hope of obtaining some suggestions about changes in associative structure with age. My first question could have been rephrased in various ways: Are there age differences in associative structure? Is the associative structure of adults more firm and rigid than that of children? Does the number of dimensions increase with age? Are there age differences in the types of dimension: Is the associative structure of children dominated by functional or part-whole relations, and that of adults by verbal abstraction such as superordination, coordination, etc.?

2. Dr. Bilodeau's analysis deals with the important problem of discrete vs. continuous associations, or more generally, with that of inter- vs. intra-individual associative hierarchies. Studies in verbal learning and verbal behavior concerned with natural language have relied almost exclusively on group norms of associations. But it is by no means clear whether such group norms adequately reflect the associative hierarchies of individuals. At least in part, this problem is a methodological and statistical one. In the normative studies of discrete associations each individual presumably emits a response that is highest in his own response hierarchy; in studies of continuous associations of single individuals each S is bound to go down in his response hierarchy from his primary, to his secondary, and to his tertiary response, etc. Thus, each individual distribution

is rectangular, and we should expect flatter response distributions if we average over all the individual distributions, than if we rely on normative data obtained with discrete association tests.

These expectations have been empirically confirmed by Cofer (1958) and others for adult Ss, but no comparisons have been made between different age groups. We know, however, that inter-individual response variability increases with age, (Riegel and Riegel, 1964, 1965). Hence, we may ask whether this increase is due to greater intra individual variations, or to increasing differences between the individual response hierarchies, or to both. In other words, is the increase in inter-individual response variability due to increases in vocabulary specialization or to vocabulary growth?

3. The last question raised is related to that on the interaction between developmental and cultural changes. If we detect differences in associative responses given by 6th graders in 1916 and 1962, we can be quite certain that these differences are due to cultural changes such as changes in education. However, if we detect differences between 6th graders and adults either in 1916 or in 1964 we cannot be sure whether these differences are due to age or to cultural background. Adults differ from children in both their age and their cultural background. Drs. Jenkins, Russell, and Palermo (1960, 1965) and Riegel (1965) have discussed historical changes in word associations, and Palermo and Jenkins (1965) discussed age differences, which are, of course confounded with historical changes.

4. Dr. Palermo has also directed our attention to age differences in the preferred types of associations, where by types of associations we mean such restricted associations as superordinate, part and function-responses. At various other occasions during our meeting we discussed age differences in paradigmatic and syntagmatic associations. Unfortunately, this distinction remains unclear. In the most general sense, paradigmatic associations are defined as responses of the same form class as the stimulus, while syntagmatic associations are defined as responses of a different form class than the stimulus. In a narrower sense, however, paradigmatic associations are regarded as potential substitutes for the stimulus in linguistic context and syntagmatic associations as being close to the stimulus in linguistic context, but not occurring in the same slot.

Dr. Palermo's statements on the superordinate-shift are pertinent to this problem since they point to clear limitations in the distinction between paradigmatic and syntagmatic associations. Quite obviously, similars and, to a lesser extent, coordinates and superordinates can function as substitutes for the stimulus; however, part- and location-responses cannot, even though they belong to the same form class as the former types of responses and as the stimuli. Such responses fulfill syntagmatic functions (in the narrower sense). Thus, the distinction between paradigmatic and syntagmatic associations seems too general and does not provide information which is detailed enough for an analysis of developmental changes or other individual differences.

5. Related to this last problem is a final comment which may reflect a conceptual bias rather than an empirical question. There is a question as to whether associations are ever free. G. E. Müller has expressed the opinion that individuals approach a state of free associations only when they are under hallucinations, have flights of ideas or pathological perserverations. Under normal circumstances either the environment or the individual himself imposes constraint upon his verbal output or reactions; that is, he is guided either explicitly or implicitly by cognitive and non-cognitive sets and task conditions. Individuals are not really free in free associations and thus, it is not surprising that certain groups of Ss, for instance, children or old adults, show systematic preferences for certain kinds of responses.

If we accept the viewpoint that associations are never free, it seems reasonable to manipulate the degree of constraint by instructions or task conditions. Instead of asking for any word that comes into S' mind we may ask for superordinates, usages, nouns or part-responses. Such variations will inform us about Ss abilities to distinguish between such different response classes, as the paradigmatic-syntagmatic classes and the semantic classes described above. Moreover, such procedures will allow us to study the interdependence of these classes and, thus, once more the conceptual structure underlying the associative behavior of various groups of Ss.

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LANGUAGE PERCEPTION AND DISCRIMINATION

Thursday Morning, October 21, 1965

Irwin Pollack and Ronald S. Tikofsky (Chairmen)

Descriptions of the vocalization of infants prior to the onset of "true speech" have heretofore been based primarily upon qualitative analyses. The two papers presented in this session are illustrative of recent attempts to increase quantitative precision in the evaluation of infant vocalizations. The two papers represent distinctively different approaches to this problem. Markel and his co-workers derive their approach from current phonetic theory. They define eight "distinctive features" of infant utterances which they believe characterize infant speech. Through standardized training procedures, they are able to achieve high reliability among judges in evaluation of infant utterances. On the other hand, Lane and Sheppard avoid the problem of human judgment by employing a high-speed digital computer to analyze selected prosodic features of infant speech. The speakers served as consultants for Study C: Development of Speech Sound Specificity in Children (Donald J. Sharf and David T. Prins) and for Study D: Development of Auditory Discrimination in Children (Ronald S. Tikofsky and Irwin Pollack).

A Distinctive Features Analysis of Pre-Linguistic Infant Vocalizations

Egan A. Ringwall, Hayne W. Reese, and Norman N. Markel

This is the first report of a research project studying the behavioral correlates of infant vocalizations.[1] The investigators are recording at regular intervals the vocalizations of infants beginning a few days after birth and continuing until the subjects are two years old. Speech and intellectual development of the infants are assessed when the subjects are three years of age. The ultimate aims of this project are to measure the relationship between infant vocalizations and linguistic development, and to determine the feasibility of using infant vocalizations as a predictor of later psychological and intellectual status. This report describes the method that was used to analyze the vocalizations of three-day-old infants, which were obtained in the hospital nursery. All subsequent recordings were made in the infants' homes.

Previous phonetic and spectrographic analyses of the vocal behavior of infants in their "pre-linguistic" stage, from birth to approximately four months (Irwin, 1957; Lenneberg, 1962), were felt to be either inappropriate, inordinately time-consuming, or limited in the number of relevant variables that could be obtained. The major difficulty with a phonetic transcription of pre-linguistic infant vocalizations is that the non-linguistic characteristics of these vocalizations are not identified, e.g., the length of the vocalization, the direction of the air stream, and the force of the air stream.[2] The major difficulties with using the sound spectrograph to study pre-linguistic vocalizations are the limitation of the size of any given sample of sound that can be analyzed at one time (2.4 seconds), the lack of reliable evidence for measuring acoustic variables other than frequency, intensity, and duration, and the inability to convert spectrograms of infant vocalizations into meaningful linguistic data, such as the phonemes that are being approximated by the infant.

Given these considerations, a method to code infant vocalizations was developed based on the "distinctive features" concept of Jakobson, Fant and Halle (1952). The description of any sound in terms of the following distinctive features includes all of the information given by a symbol in the International Phonetic Alphabet (I.P.A.), and in most cases it includes additional information that is not given by these symbols. However, for the acousti variables of frequency, intensity, and duration, the corresponding features are probably not as precise as would be obtained with a sound spectrograph. The eight distinctive features are: (1) vocalization (sound vs. silence), (2) length of sound (short vs. long), (3) length of silence (short vs. long), (4) direction of air stream (egressive vs. ingressive (5) air passage (oral vs. nasal),(6) muscular tension (lax vs. tense), (7) force of air stream (soft vs. loud), (8) vocal cord vibration (voiced vs. voiceless). The present paper reports the results of applying this method to the vocalizations of 40 three-day-old infants.

Method

A. Definition of the Eight Distinctive Features.

1. Vocalization (sound/silence).

Any audible vocalization produced by the infant is coded as one *sound* regardless of duration. Audible, but unobstructed, breathing is coded as *silence*. Any perceptible silence or "break" is considered the end of one sound.

2. Length of Vocalization (short/long).

Duration of the word"pit" as it is normally pronounced in isolation is used as the standard of measurement. If the sound is the same duration or shorter than "pit" the sound is coded *short*; if the duration of the sound is longer than "pit" it is coded *long*.

3. Length of Silence Between Vocalizations (short/long).

The duration of the silence between sounds is coded *short* or *long* following the same procedures as in 2, above.

4. Direction of Air Stream (egressive/ingressive).

If the air stream is expelled outward the sound is coded egressive; if the sound is produced by inhalation of air it is coded ingressive.

5. Air Passage (oral/nasal).

If the air stream passes through the oral cavity, with complete or partial velvic closure, the sound is coded *oral*; if the velum is lowered and the oral cavity closed at some point, so that the air stream passes through the nasal cavity, the sound is coded *nasal*. By definition, therefore, sounds which are partially nasal would be considered *oral*.

6. Muscular Tension (lax/tense).

If the sound is made with relatively relaxed muscles in the mouth and throat it is coded lax; if the sound is made with relatively tense muscles in the mouth and throat it is coded tense.

7. Force of Air Stream (soft/loud).

The loudness of adult conversational speech is used as the standard of measurement. If the sound is equal to, or softer than this standard it is coded soft; if the sound is louder than this standard it is coded loud.

8. Vocal Cord Vibration (voiced/voiceless).

If the sound has any quality of pitch caused by vocal cord vibration it is coded voiced, if the sound has no quality of pitch it is coded voiceless.

B. Transcription Sheet

The following is a sample of the transcription sheet that is used to code the eight distinctive features. Vocalizations are indicated by checking the Arabic numerals. Beneath each Arabic numeral the distinctive features of that particular sound are coded: length of vocalization (s/l); direction of air stream (e/i); air passage (o/n); muscular tension (l/t); force of air stream (s/l); vocal cord vibration (v/u). Length of the silence between vocalizations is coded (s/l) between the Arabic numerals.

Segment No._______

1	s/l	2	s/l	3	s/l	4	s/l	5	s/l	6	s/l	7	s/l	8	s/l	9	s/l	10	s/l	11	s/l
s/l		s/l		s/l		s/l		s/l		s/l		s/l		s/l		s/l		s/l		s/l	
e/i		e/i		e/i		e/i		e/i		e/i		e/i		e/i		e/i		e/i		e/i	
o/n		o/n		o/n		o/n		o/n		o/n		o/n		o/n		o/n		o/n		o/n	
l/t		l/t		l/t		l/t		l/t		l/t		l/t		l/t		l/t		l/t		l/t	
s/l		s/l		s/l		s/l		s/l		s/l		s/l		s/l		s/l		s/l		s/l	
v/u		v/u		v/u		v/u		v/u		v/u		v/u		v/u		v/u		v/u		v/u	

C. Time Signals.

A major obstacle in coding infant vocalizations is the difficulty of keeping track of the vocalizations, both while the individual coder is transcribing and also in attempting to compare the transcriptions of two coders. Contrary to language, there are no "words" on which the transcriber can focus. To overcome this difficulty a copy of the original recording is made and a consecutive number read on to the copy at approximately every five seconds. This division of the recordings into five second "segments" has proven to be an excellent solution to the problem of focusing on any given vocalization.

D. Training Tape.

After the eight distinctive features had been operationally defined, two coders independently coded the recordings of four of the infants.[3] The infants used in this stage were selected impressionistically to represent different types of vocalizations after the coders had listened to the entire sample of the recordings of the three day old infants. A training-tape was then made containing (1) samples, out of context, contrasting the distinctive features, and (2) the actual vocalizations of four infants segmented by time signals. A manual with the "correct" transcription was prepared to accompany this tape. In all, the training tape consists of 39 segments of four infants, with a total of 216 vocalizations to be coded.[4]

E. Subjects.

The subjects for this study were selected from the sample of infants entering the Collaborative Study of Cerebral Palsy and Other Neurologic and Sensory Disorders of Infancy and Childhood, at the Children's Hospital, Buffalo, New York.[5] All subjects met the following criteria: white native-born parents; birth weight 2500 to 400 grams; 37 to 42 weeks gestation period; rating of 7+ on the Apgar scale of neonates; no placental separation; heart beat of 120 to 160; and, no jaundice or erythroblastosis.

F. Recording.

A Wollensak Model 1515-4 portable tape recorder with the standard accessory microphone was used. Although the fidelity of this tape recorder is not as good as some available, it seemed the best suited for our purposes, primarily because of its mobility. Female assistants were used to make all recordings to allow minimum interference in the infant's homes, and the size and weight of higher fidelity tape-recorders would have made these visits extremely difficult. Vocalizations of the three-day-olds reported in this paper were all made in the nursery at the Children's Hospital, Buffalo, New York. A room was provided adjacent to the nursery, and the babies were wheeled into this room in their basinettes. The microphone was hooked to the side of the basinette facing the infant. If for the first five minutes the infant had not produced any vocalizations, the experimenter "flicked" the infant's foot and allowed the recorder to run for another five minutes.

Results

A. Reliability of Coding

Two estimates of the reliability of the coding procedure were obtained. In the first, the results of two coders working independently were compared. In the second, the final transcription agreed upon by one team of two coders was compared to the final transcription agreed upon by a second team of two coders. The second team of two coders had no specialized training in linguistics, and had been taught the coding procedures by listening to the training tape and following the manual. For both estimates of reliability the final transcriptions for one infant were compared.

An important aspect of the coding procedure should be noted at this time. The first feature to be coded is vocalization (sound/silence). After each coder has determined, independently, the number of sounds in one segment, the transcriptions are compared. At this point the coders attempt to arrive at agreement as to the number of sounds in each segment, to make certain they are describing the distinctive features of the same sounds. This procedure must be followed if the description of the distinctive features of the sounds are to be compared. Disagreements as to the number of sounds arise primarily because (1) there are often sounds on the tape which are difficult to categorize as produced by the infant or as "noise"; (2) some sounds are so faint that a coder may fail to hear them until attention is called to them; and (3) the "break" in a sound may be so brief that it is not noticed until attention is called to it. Because of these problems a separate estimate of the reliability of determining the number of sounds is reported.

1. Reliability of Two Coders.

a. Number of Sounds.

The total number of sounds recorded by both coders was 153, and there were 22 disagreements. A disagreement in this instance means that one coder indicated a sound where the second coder did not. If both coders agreed that there was silence for an entire segment it was counted as one agreement. There were four such segments on this tape, which means there was a total of 127 sounds to be coded. Since both coders agreed on 131 out of the 153 sounds there was 86 percent agreement between the two coders for indicating the occurrence of sounds.

b. Distinctive Features.

After reaching agreement on the number of sounds in each segment, the coders independently coded the distinctive features of each sound. Since there were 127 sounds, a decision about each of the 7 other distinctive features had to be made 127 times, for a total of 889 decisions. Table 1 indicates the percentage of agreement for each distinctive feature.

2. Reliability of Coding on Two Occasions by Two Different Teams of Coders.

a. Number of Sounds.

The total number of vocalizations (sound/silence decisions) agreed upon by the two coders on Occasion 1 was 143, and the total number of sounds agreed upon by a second team of two coders on Occasion 2 was 131, indicating 92 percent agreement as to the number of sounds.

b. Distinctive Features.

Although the percentage of disagreement in coding the number of sounds is small (8%), what disagreements there were made it difficult to compare the coding of the distinctive features in many segments. That is, if one team had coded 5 sounds in a given segment and the second team had coded 6 sounds for that segment, it was impossible to compare the coding of the distinctive features for that segment. There were, however, 45 sounds that could be directly compared from the transcripts, and Table 2 indicates the percentage of agreement between the two teams of coders for these 45 sounds.

Table 1

Percentage of Agreement for Coding the Distinctive Features of 127 Sounds for Two Coders

Distinctive Feature	No. Agreements	Percentage Agreement
Length of Sound	124	98
Length of Silence	121	95
Direction of Air Stream	118	93
Air Passage	127	100
Muscular Tension	110	87
Force of Air Stream	117	92
Vocal Cord Vibration	116	91
Total No. Decisions	889	94

Table 2

Percentage of Agreement Between Two Teams of Coders on Two Different Occassions Coding the Distinctive Features

Distinctive Feature	No. Agreements	Percentage Agreement
Length of Sound	45	100
Length of Silence	39	87
Direction of Air Stream	43	96
Air Passage	45	100
Muscular Tension	38	84
Force of Air Stream	41	91
Vocal Cord Vibration	40	89
Total No. Decisions	315	92

B. Preliminary Normative Data

The distinctive feature analysis described above was applied to the vocalizations of 40 three-day-old infants. The recordings were sampled so that the first, third, fifth, seventh, and ninth sequence of ten consecutive segments were coded. Thus, segments 1-10, 21-30, 41-50, 61-70, and 81-90, were included. Since each segment is approximately 5 seconds in length, each sample consists of at least 50 seconds of continuous recording, and the results reported are based on at least 4.2 minutes of recording for each infant. Table 3 indicates the results obtained for 40 three day olds.

Discussion

This paper describes a method for coding the vocalizations of infants in the pre-linguistic stage of development. The introduction indicated the reasons for developing a coding technique based on distinctive features, rather than using phonetic or spectrographic analyses, but we do not view a distinctive features analysis as mutually exclusive of either of these methods. Irwin's studies (1957), and a preliminary examination of our own tapes indicate that at about three to four months of age a classic phonetic transcription can be meaningfully applied to infant vocalizations. We plan to carry our distinctive features analysis beyond this stage, and through this method we should be able to see the development of the phonetic system in the shifts in the kind, number, and combinations of distinctive features. Lenneberg (1962) has indicated that in addition to frequency, intensity, and duration, he can discriminate vocal cord modulation, modulation by constriction of the air tunnel, modulation by resonance, and modulation by interruption of the air stream, from spectrograms. Again, if there is validity to a distinctive features analysis there should be some correlation between the distinctive features and the spectrographic data. It may be that the application of both the spectrograph and distinctive feature analysis will yield the maximum amount of information concerning pre-linguistic infant vocalizations.

At the present time the distinctive features analysis has produced data that will be meaningful to the study of the relationships between infant vocalizations and later linguistic and psychological development. The results indicate that a distinctive features analysis is reliable, that it yields normative data on the quality and frequency of infant vocalizations, and, that it provides measures of individual differences between infants in the pre-linguistic stage.

Table 3

Normative Data on 40 Three Day Olds Based on 4.2 Minutes for Each Infant

Sub. No.	Vocalization No. sounds*	Silence % short	Sound % short	Direction % egressive	Passage % oral	Tension % lax	Force % strong	Vocal Cords % voiced
1	4.10	60	66	94	100	14	67	78
2	0.62	29	94	90	97	35	100	71
3	5.90	94	48	59	100	18	45	86
4	0.28	7	79	71	64	29	100	86
5	2.10	61	92	78	96	21	85	78
6	3.20	72	91	88	84	52	95	57
7	1.50	38	100	97	96	36	100	41
8	4.00	88	76	91	95	22	79	80
9	1.70	57	90	92	92	49	98	56
10	2.10	73	68	94	98	45	99	56
11	5.60	77	33	80	90	4	34	82
12	2.10	33	99	98	89	53	100	33
13	3.30	92	65	64	100	5	68	95
14	0.60	37	97	93	37	63	100	27
15	2.00	64	79	86	92	29	66	79
16	1.30	33	97	94	100	85	100	25
17	2.30	48	92	80	99	52	100	60
18	2.20	74	91	76	100	62	93	45
19	3.70	91	63	74	99	5	51	93
20	3.80	93	63	90	100	14	56	86
21	1.90	60	91	98	97	65	99	55
22	1.60	38	100	84	100	82	100	24
23	1.10	13	98	100	100	95	100	20
24	1.20	34	91	83	84	83	91	19
25	0.66	48	76	100	100	70	94	45
26	3.90	78	84	67	96	21	72	84
27	1.50	57	99	95	97	77	100	22
28	4.60	82	62	76	99	17	40	81
29	0.76	29	95	89	95	66	100	37
30	1.40	66	99	35	96	30	100	10
31	1.40	58	86	90	100	42	94	79
32	6.20	99	54	59	100	00	8	99
33	3.50	73	76	81	93	23	69	69
34	0.34	35	94	94	82	47	100	41
35	6.50	97	48	73	100	2	55	98
36	0.34	24	100	76	94	53	100	53
37	0.30	20	100	100	73	45	100	73
38	0.74	54	62	100	97	89	100	95
39	4.20	91	54	76	99	1	19	82
40	0.34	47	100	76	100	94	100	6

* Mean number of sounds per 5 second segment.

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Footnotes

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2. It is interesting to note that another research project studying pre-linguistic infant vocalizations has independently come to the same conclusion regarding the use of phonetic transcriptions (Bullowa, Jones, and Bever, 1964, p. 105).

3. The authors are indebted to Miss Susan Cohen, our research technician and analyst, for her assistance in developing the coding procedures.

4. The training-tape and manual are available upon request.

5. The authors would like to thank Dr. Milton Westfall, Director, Collaborative Project, Children's Hospital, Buffalo, New York, for his assistance in obtaining the subjects for this study.

Development of the Prosodic Features of Infants' Vocalizing

Harlan Lane and William Sheppard
Center for Research on Language and Language Behavior

The vocal behavior of an infant during the first few months of life is the matrix for later language development. Therefore, the differentiation and organization of infant vocalizing as a function of maturational and environmental factors holds considerable interest.

Previous research in this area consists almost entirely of descriptions by a single observer transcribing the utterances of a single infant in naturalistic settings. Two abridged biographical reports will be samples: one from a "pre-objective" period, that of the late 19th century, and one from recent decades, employing "more refined techniques" (McCarthy, 1946). The earlier writer, a keen observer of behavior, is Charles Darwin. The contemporary writer, a linguist, is W. F. Leopold.

"The noise of crying is uttered in an instinctive manner... After a time the sound differs as to the cause, such as hunger and pain...he soon appeared to cry voluntarily... When 46 days old he first made little noises without any meaning to please himself, and these soon became varied....At exactly the age of a year, he made the great step of inventing a word for food, namely mum...and now, instead of beginning to cry when he was hungry, he used this word in a demonstrative manner...implying 'Give me food'" (Darwin, 1877, p. 285).

"During the first few weeks the only sounds produced were cries of dissatisfaction... in the seventh and eighth weeks the sounds ceased to be purely incidental. She uttered more arbitrary sounds of satisfaction...cooing as an articulated expression of feelings of satisfaction was therefore well established by the end of the second month... By the seventh month there was a good deal of babbling prevalently ranging from [a] to [ɛ], long, without many tongue movements... At the end of the eleventh month, her active vocabulary consisted of two words" (Leopold, 1939, p. 72).

Linguistic studies in this field have focused on providing a description of the language of a particular child at different levels of development. The units of analysis have been phonemes, morphemes, words and sentences. The procedures for data collection usually begin with phonetic transcription of the infant's vocal behavior by a trained observer. The most extensive studies of this type are those of the linguists Gregoire, Leopold, Cohen, and Velten. The work of Irwin and Chen in the 1940's is notable for the refinements they introduced. These investigators began the practice of using two observers rather than one; this allowed them to obtain measures of observer agreement. They also increased the numbers of subjects observed and selected a more adequate sample of subjects.

Psychologists working in this area have tended to study more molar aspects of language development. The main interest has been to provide normative data on various indices of development, e.g., use of form class, size of vocabulary, mean sentence length, and the like. The procedure for data collection is again transcription (usually alphabetic) by a trained observer using cross-sectional and longitudinal sampling. Among the classic studies of this type are those of Davis, Fisher, McCarthy, Shirley, Templin, and Lewis.

The most striking feature of the research literature on infant vocalizing is the lack of advancement in the field. The basic drawback is the reliance on transcriptions obtained by observations in naturalistic settings. Consider the difficulty of transcribing infant speech sounds. Because the infant is in the process of learning to articulate, the sounds he utters are unlikely to fit neatly into any classificatory system. There is the danger that the trained observer filters the variagated vocal behavior through his own classificatory categories--categories developed with adult vocal behavior--and thus rejects much of importance. Moreover, the infant utters sounds rapidly and sporadically, making it difficult for the observer to keep an accurate or complete record.

The obvious sampling problems have also limited the generality of the findings of previous studies. It has been recognized that when investigator and subject are also mother and child experimental rigor receives little nourishment. In those less numerous studies in which the investigator has intruded into the home, the schedule of observation and transcription usually has been to be most charitable, unsystematic. In summing up the results reported up to 1941 Irwin says:

"It will be apparent from this review of the more important studies...that there does not exist a large body of data secured from adequate samplings of infants for purposes of a statistical analysis... Usually no systematic research methods were formulated; statistica techniques essential to the analysis of mass data are practically absent, no reliabilities of observers have been established, many observers used alphabetical rather than phonetic systems of symbols for recording; and most reports indulge in an inordinate amount of interpretation supported by very little empirical material." (Irwin, 1941, p. 285).

In her classic review of the literature on language development in the child, McCarthy writes: "Although this wealth of observational material has proven stimulating and suggestive for later research workers, it has little scientific merit, for each of the studies has employed a different method, the observations have for the most part been conducted on single children who were usually either precocious or markedly retarded in their language development; the records have been made under varying conditions, and most of the studies are subject to the unreliability of parents' reports" (McCarthy, 1946, p. 478).

Attempts to ameliorate one or more of the methodological problems that we have reviewed have waited upon advancements in instrumentation. In Part II of her 1929 review McCarthy described some of the early attempts to record speech by means other than transcription by an observer. The first device she described was the manometric flame, invented by Koenig in 1862. With this device, the flame of a gas jet is disturbed by the sound wave and these disturbances are recorded photographically. The phonautograph, invented by Scott in 1859, recorded the speech wave by means of a stylus attached to a diaphragm. The best device of this general type was the phonodeik, which consisted of a horn and a diaphragm; a small platinum wire attached at one end to the diaphragm was passed around a jewel-mounted spindle to a delicate spring. Attached to the spindle was a tiny mirror which reflected a fine beam of light onto a moving photographic film. The phonodeik could respond up to 10,000 cps but the horn and diaphragm introduced a certain amount of distortion.

In 1893 Blondel had devised the oscillograph and with improvements in amplifiers it promised to be a very useful piece of apparatus. However, even before the oscillograph had been developed to the point where it gave a practically perfect representation of the sound wave, it became clear that, once recorded in all its complexity, the waveform was virtually impossible to analyze in ways that were useful for studying speech.

Sound recording devices, the phonograph and later the wire and tape recorders, even when developed further than they were in 1929, solved only part of the problem. The actual speech sounds could then be recorded and replayed but an observer still had to rely on his judgment as a perceiver of sound and speech in order to analyze the data.

Two years after the introduction of the sound spectrograph, in 1949, Lynip published a report of the use of this device in the study of infant speech. He recorded the speech sounds of a little girl, beginning with the birth cry and sampling at intervals ending at 56 weeks when intelligible speech began. A spectrographic analysis of these recordings revealed nothing which resembled the spectrograms of phonemes produced by adults.

In 1960, Winitz published a data-garnished polemic on the subject of the spectrographic analysis of infant vocalization. He argued that the fact that the infant's spectrograms didn't look like spectrograms of adult speech simply proved that the spectrograph isn't a good device for the study of infant vocalizations. "The basic data against which any instrumental method of phonetic analysis must be validated are the phonetic analyses made by competent observers whose validity Lynip questions" (Winitz, 1960, p. 173).

Without resolving the question of the validity of spectrographic description, it must be acknowledged that for each two-second sample of vocalization approximately fifteen minutes are required to process, calibrate, crudely quantify, and classify each spectrogram--and that the problems of observer interpretation remain, although transferred from auditory to visual modes.

This brief account of the techniques that have been employed so far for collection and acoustic analysis of infant vocal behavior indicates that an extension of our knowledge of vocal development requires new techniques. Accordingly, the Center for Research on Language and Language Behavior undertook a year ago to collect permanent, complete and continuous records of all vocalizations of two infants, and then to process these records by novel electro-acoustic techniques.

Recording and sampling of vocalizing. During the deliveries of the infants whose vocalizing is the subject of this study, medical personnel wore lapel microphones whose outputs were recorded on magnetic tape. All subsequent vocalizing by the infants at the hospital was recorded by placing them in private rooms containing a microphone wired to a fast-acting voice-operated switch (Miratel) and to a tape recorder (Tandberg). After leaving the hospital, both children were cared for at home in plexiglass "air cribs" (T.M.I.) that provided no sources of sound within the crib and attenuation of external sounds--hence, a good recording environment. The parents of the children (research assistants at the Center in both cases) were paid to keep a detailed record on prepared forms of major environmental events affecting the infant. These records were synchronized with the tape recordings by writing down the reading of the footage indicator on the tape recorder.

Complete recordings of all vocal behavior during the first five months of life constitute a formidable tape library, which was sampled for analysis in the following way. A master tape was prepared for each child which contained three 95-sec samples of the vocal behavior during every fourth day of life for the first 141 days. For the samples taken from the first month (in which the infant had no regular sleeping times) the three daily samples were excerpted from the recordings for 12 a.m. to 8 a.m., 8 a.m. to 4 p.m., and 4 p.m. to 12 p.m., respectively. This was accomplished by listening to the recording, beginning at the start of each period, and copying the first 95 sec onto the master tape. In some cases undesired noises intruded and the first 95 sec excluding these intrusions was copied. For the samples of vocalizing in the following three months, the three daily periods from which 95-sec samples were taken were: time at awakening (T) to T + 4 hours; T + 4 hours to T + 8 hours and T + 8 hours to T + 12 hours. These sampling procedures yielded 108 95-sec samples for the initial acoustic analysis.

Analysis of the prosodic features. The development of the prosodic features of the infant's vocal behavior was analyzed by extracting three acoustic parameters of the vocalizing during each of the 108 samples, using analog electronic devices. The outputs of these parameter extractors were sampled every 25 sec by an analog-to-digital converter, then processed by an on-line digital computer (PDP-4, Digital Equipment Corp.).

The changing fundamental frequency of the vocalizing was extracted by filtering tape-recorded signals into two frequency ranges. Since the harmonics of the fundamental frequency often have more energy than the fundamental itself, a range-control voltage is generated when there is energy in the lower range which turns off the upper range to exclude the harmonics. If no energy exists in the lower range, however, the fundamental frequency in the upper range is processed unimpeded. In either case, the nearly sinusoidal output from the filters is amplified in a mixer and read on a frequency meter which provides a DC voltage output proportional to the frequency of the fundamental sine wave at its input. A DC amplifier then adjusts the voltage range and polarity for input to the computer.

The changing amplitude envelope of the vocalizing was extracted by applying the recorded signals to a full-wave rectifier followed by a low-pass filter. The output of this device is a DC voltage that is proportional to the absolute value of the amplitude of the vocal waveform (integrated over approximately one period of the fundamental).

The duration of each utterance within a sample, the third prosodic parameter, was determined in the computer by processing the input from the amplitude extractor. When the amplitude dropped below a threshold value and remained there longer than the silence threshol ($\underline{t}_o$) four out of five samples, the end of an utterance was logged at the time of the initial drop. The start of a new utterance was recorded when the amplitude exceeded threshol again.

In addition to defining the beginning and end of utterances, the computer performed the following preliminary processing. Whenever the amplitude fell below a minimal threshold value $\underline{a}_o$, or the frequency fell below a minimal value $\underline{f}_o$, in a 25 msec sample, the values of $\underline{a}$ and $\underline{f}$ were set to zero. This eliminated spuriously low readings due to noise as well as vocal sounds without voicing at the glottis and hence without prosodic value. It also eliminated false frequency readings that would result from the rise-decay time of the frequency meter in response to instantaneous onset or cessation of voicing.

After sampling and then correcting the amplitude and frequency inputs in this fashion, the digital values were reconverted to voltages and plotted as a function of time on a strip-chart recorder. These records of the amplitude and frequency contours after preliminary processing were compared with those obtained directly from the parameter extractors (before computer processing) so as to choose values of a_o, f_o, and t_o that did not distort the original records.

After the preliminary processing, the computer determined, for each 95-sec sample, the number of utterances as defined above, the duration of each utterance; and the mean and standard deviation of the fundamental frequency and amplitude of each utterance. Pooling these statistics for each of the utterances in a sample, the computer determined next their frequency distributions over the entire sample. These frequency distributions were found to be highly right-skewed. A logarithmic transformation was then applied in order to eliminate the skewness and thus to normalize the distributions. Hence, all statistics were computed using the logarithms of the frequency, amplitude or duration values. The computer determined next the means and standard deviations associated with these transformed composite distributions. Consequently, there were two kinds of statistics reported for each 95-sec sample: (1) within utterance measures of central tendency and variability, averaged over utterances, and (2) between utterance measures of central tendency and variability. All in all, these composite statistics for each sample were printed out along with their frequency distributions:

(let M = mean, S = standard deviation, f = fundamental frequency, a = amplitude, d = duration.)

M(MF) - overall fundamental frequency

S(MF) - variability between utterances in fundamental frequency

M(Sf) - overall variability within utterances in fundamental frequency

S(Sf) - variability between utterances in the variability within utterances in fundamental frequency

M(Ma), S(Ma), M(Sa), S(Sa) - as above but for the amplitude parameter

Md - mean utterance duration

Sd - variability in utterance duration

These statistics, describing the three prosodic features of the vocalizing in each sample, are then plotted separately as a function of age at time of the sample, with the time of day (in three intervals) as a parameter. In this way, developmental trends may be discerned in the prosodic features of the infant's vocal behavior.

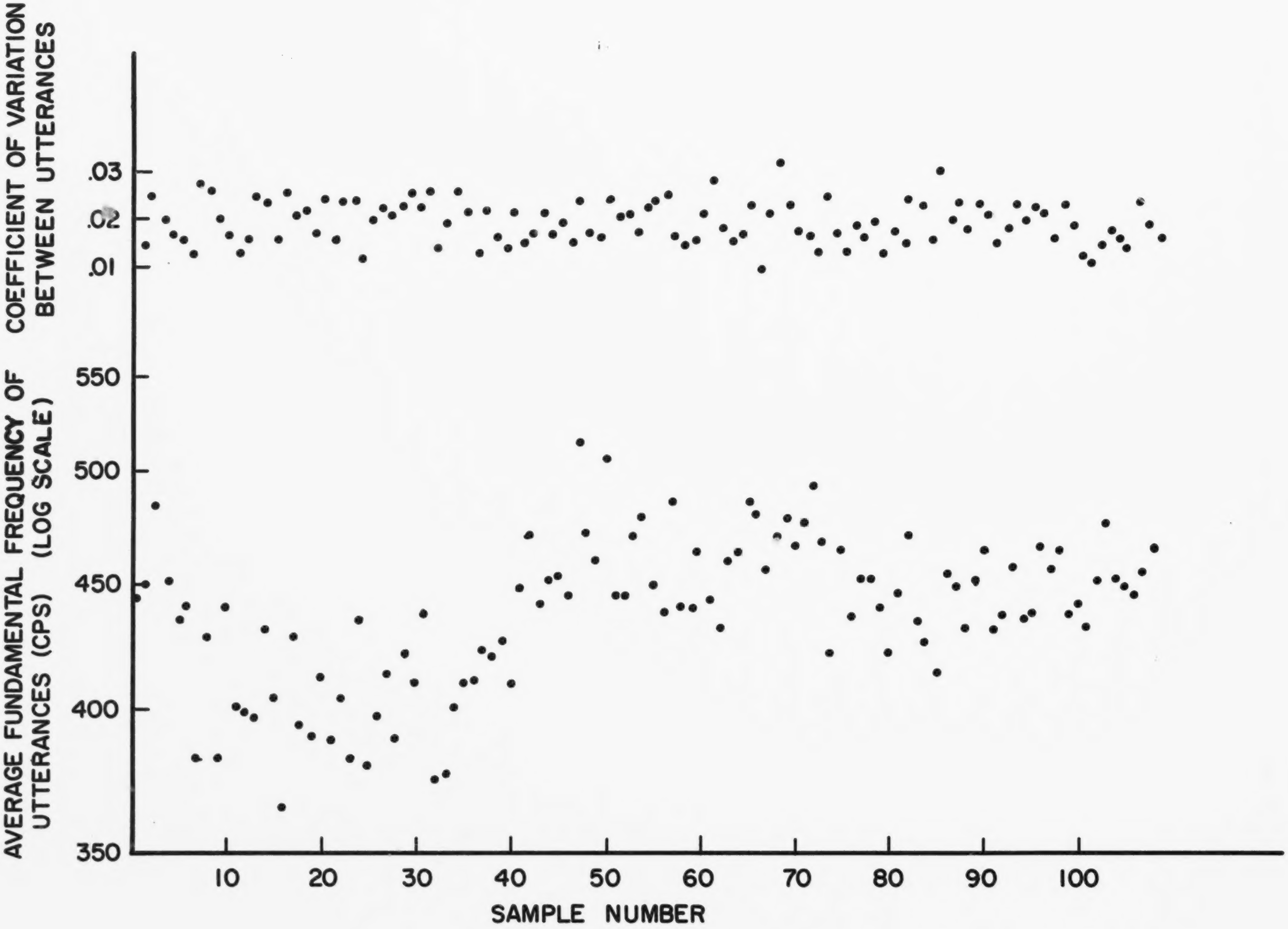
COEFFICIENT OF VARIATION BETWEEN UTTERANCES
.03
.02
.01
AVERAGE FUNDAMENTAL FREQUENCY OF UTTERANCES (CPS) (LOG SCALE)
550
500
450
400
350
10
20
30
40
50
60
70
80
90
100
SAMPLE NUMBER

FIGURE 1

Results. In Fig. 1 the average fundamental frequency of utterances, M(Mf), and the coefficient of variation between utterances in fundamental frequency, CVF_{bU}, [=S(Mf)/M(Mf)] are presented as a function of sample number. An examination of the developmental changes over the first 108 samples (141 days) shows that the average fundamental frequency M(Mf) at birth was approximately 450 cps, that it decreased to 370 cps by sample number 33 (approximately 45 days), and that it then rose and stabilized at about 450 cps for the duration of the study. The coefficient of variation between utterances remained small and constant at between .01 and .03 over the entire study.

Insert Fig. 1 about here

In Fig. 2 the average duration of utterances in msec and the coefficient of variation of duration (CVD_{bU} = Sd/Md) are presented as a function of sample number. The average duration ranges from 100 msec to 800 msec over the 108 samples. No developmental trend is apparent although the variability from sample to sample does decrease with age. The coefficient of variation of duration remains constant at about .20 over the entire sample.

Insert Fig. 2 about here

Further examination of developmental trends awaits the completion of further statistical computations now in progress with the present data.

Footnote

[1] This paper was presented at the First Symposium of the Development of Language Functions, sponsored by the Center for Human Growth and Development, University of Michigan, on October 20-22, 1965.

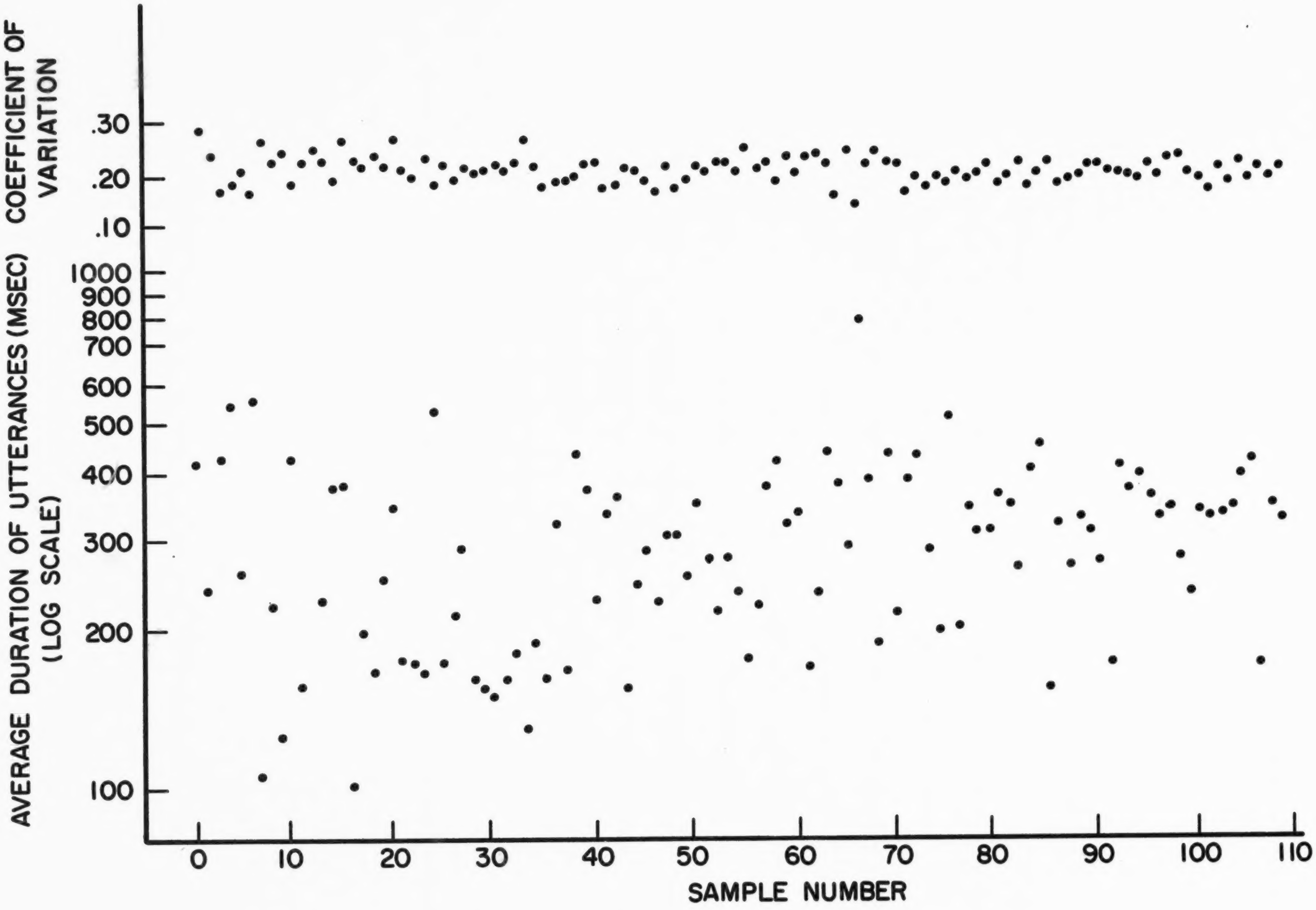
COEFFICIENT OF VARIATION
.30
.20
.10
AVERAGE DURATION OF UTTERANCES (MSEC) (LOG SCALE)
1000
900
800
700
600
500
400
300
200
100
0
10
20
30
40
50
60
70
80
90
100
110
SAMPLE NUMBER

FIGURE 2

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Discussion and Comments: Language Perception and Discrimination

Summarized by Irwin Pollack and Ronald S. Tikofsky

The two papers are, in one respect, very different but, in another respect, very much alike. Both employ analytical tools which have proved useful in current phonetic and linguistic research. In this way, both studies attempt to set the stage for the later tracing of the developmental sequence of vocal utterances. This is not to say that infant grunts and groans must necessarily be the raw materials for later speech development. But, rather, if there is a continuity of development of vocal behaviors, we might stand a better chance to trace the continuity of vocal behaviors in terms of units which have proved useful in the analyses of adult speech than with variables unrelated to adult speech.

The two papers, however, differ significantly in their approach to the analysis of infant vocalizations. Ringwall, Reese, and Markel's paper is concerned with an analysis of phonetic-like features by trained listeners. The phonetic-like features bear a resemblance to the "distinctive features" employed by Jacobson, Fant, and Halle (1952) in their classical phonetic analysis of adult speech. It should be pointed out, however, that the specific distinctive features employed by Markel and his colleagues, are not necessarily the same features employed for the analysis of adult speech--despite their common names. At the present stage of development, Markel is correctly most concerned with problems of reliability. Questions related to validity must necessarily be deferred until the reliability of the procedures can be established.

Lane and Sheppard, by appropriate choice of well-defined acoustical parameters and their judicious employment of measurement instruments, have avoided the problem of the reliability of human judgment. Since Lane and Sheppard took elaborate protocols of the infant's other behavior, these workers may also be able to determine the effect of various situational variables (e.g., time since last meal) upon the various parameters of infant vocalization.

Both approaches must eventually face up to the extremely difficult problem of segmentation of utterances. The segmentation of speech remains unsolved for adult speech, although the problems may be less severe for infant speech. Ringwall, Reese and Markel note the difficulty of judging the number of sounds. Lane and Sheppard define their segment in terms of a drop in amplitude below an arbitrary threshold for a defined time period. They note the necessity of choosing their thresholds with care in order not to distort the records.

Both approaches are now in the early stage of data acquisition as part of a long-term research committment to the study of infant vocalization. Hopefully, the results of the two different approaches will eventually complement each other in piecing together the mysteries of early speech development. This area is, indeed, fortunate in having a multipronged attack upon a single problem.

VERBAL STRUCTURES

Thursday afternoon, October 21, 1965
David McNeill (Chairman)

The two speakers in this session of the Conference are concerned with the development of syntax in children, an interest that can safely be attributed to Roger Brown, with whom both have worked. Dan Slobin, after an undergraduate career at Michigan, went first to Harvard, participating there in Roger Brown's project, and then to the University of California at Berkeley. Among his special gifts is fluent knowledge of Russian, his double linguistic competence benefiting us in the form of an especially valuable paper. Ursula Bellugi has worked with Roger Brown for the past five years, observing children acquire language, writing grammars of what she observes, and enlightening us all on what transpires during those years of frenetic activity when linguistic competence arises. For those familiar with the literature of this field, she will be recognized as the Urler of Urler suitcase, a sentence produced by Brown and Bellugi's two-year-old Adam. Both speakers had been invited as consultants for our Study A: The Transactional Study of Grammatical Development (William P. Livant and Wilbur A. Hass) and Study K: Longitudinal Study of Very Early Language Acquisition and Experimental Comparisons of Speech Comprehension and Production (David McNeill).

Grammatical Development in Russian-Speaking Children

Dan I. Slobin

The striking recent advances of developmental psycholinguistics have been based on careful study of the acquisition of English as a native language. The engrossing debate in regard to innate factors in language acquisition however (McNeill, in press) may be illuminated by cross-linguistic comparisons of child language. Unfortunately, extensive data on the acquisition of non-Indo-European languages are not yet available; there is, however, a sizeable Soviet body of literature which is worthy of the attention of American psycholinguists. Although Russian is also an Indo-European language, it is sufficiently different from English--most clearly in its highly inflectional grammatical structure--to serve as a useful contrast case to sharpen notions of universal aspects of language acquisition and linguistic competence.

In order to make the discussion intelligible to a non-Russian-speaking audience, a few words about the grammatical structure of the language are in order. Russian has three genders and six cases; nouns, adjectives, and pronouns show gender, case, and number. Verbs are conjugated for person and number, and, in the past tense, also for gender of subject noun. Verbs are marked for tense (three tenses) and aspect (perfective-imperfective, and, for verbs of motion, also determinate-indeterminate). There are many participial forms. The morphology is highly productive, and freely-used suffixes of many sorts abound (e.g. diminuitive, augmentative, endearing, pejorative, agentive, and so on). Word order is much freer than in English.

The most careful and intensive longitudinal study of a child's language development ever published anywhere is probably the monumental work of Aleksandr N. Gvozdev (1961), a Soviet linguist and teacher. He kept a diary of the speech of his son, Zhenya, almost daily for the first few years of the child's life, and recorded his language extensively until the age of nine (1921-1929). The following discussion is based primarily on the speech of Zhenya, supplemented with data from psycholinguistic experiments with preschool children.

Early Syntax

The beginning stages of syntactic development look very much like those of English-speaking children described by Braine (1963a), Brown and Fraser (1963), and Miller and Ervin (1964). There is clearly a small class of "pivot words" (P) and a large open class of words (O), which can be combined into three types of two-word sentences: P + O, O + P, and O + O (McNeill, in press). Gvozdev argues that these sentences are constructed, rather than imitated or memorized as units, because most of the single words appear as separate utterances, and because the two-word combinations differ from adult sentences in form.

Two-word sentences appear at about 1,8; at first there are only a few such sentences, but they become the usual utterance type by 1,9. By 1,10 they are replaced in frequency by longer sentences. As has been noted by other investigators, new pivots are often playfully practiced, the child uttering long series of pivot sentences, holding the pivot constant and substituting a variety of words from the open class (*cf*. Weir, 1962). In line with American findings, membership in both pivot and open classes is heterogeneous from the point of view of part-of-speech membership in the adult language.

The first three-word sentence is a simple negation, which involves placing a negative element at the beginning of a sentence. This is the same initial negation form found by Bellugi (in press) though the adult model in Russian often involves a double negative. For example, the adult form *nyet nikavó*[1] ("not no-one"--i.e. "there is no one") is given by the child as *nyet kavó*. *Nyet, dam* is the child's equivalent of adult *nyet, nye dam* ("no, not I-will-give"). The same negative element, *nyet*, is used in all cases, even where the adult form would have only the single negative element *nye*: e.g. instead of *nye karmí* (don't feed), the child says *nyet kamlí*. Presumably, acoustic marking singles out *nyet*, rather than *nye*, as the primordial negative element. (It should also be noted that *nyet* is the independent negative element in Russian, analogous to English *no*.)

Another source of length is the addition of content words to short sentences. Gvozdev thinks that forms learned more recently appear later in sentences, and gives the example of elaboration of one-word utterances to two-word subject-object sentences, and, with the acquisition of new verbs, to subject-object-verb sentences, although subject-verb-object order is dominant in Russian. For example, at the first stage the child may say mama; at the second, mama niska ("mama book"); and at the third mama niska tsitats ("mama book read"). (The forms are all unmarked in the child's system, and Russian does not use articles.) This subject-object-verb order is at first the dominant order in the child's speech, being replaced by subject-verb-object at about 1,11. (It is interesting to note that according to Greenberg [1963], it is apparently a linguistic universal that subject precede object in the dominant actor-action construction of a language, and that the two most common patterns are SVO and SOV.)

Word order is quite inflexible at each of the early stages of syntactic development. One might have predicted that Russian children, being exposed to a great variety of word orders, would first learn the morphological markers for such classes as subject, object, and verb, and combine them in any order. This is, however, hardly the case. Child grammar begins with unmarked forms--generally the noun in what corresponds to the nominative singular, the verb in its adult imperative or infinitive form, and so on. Morphology develops later than syntax, and word order is as inflexible for little Russian children as it is for Americans.

Arguments have been advanced by Braine (1963b) and by Jenkins and Palermo (1964) which rely upon the ordinal sequences of words in adult language to account for the order of elements in child sentences, and for the formation of word classes. Not only do the Soviet data cast doubt on these interpretations, but, as Bever, Fodor, and Weksel (1965) have pointed out, even in English, which does not make great use of inflection, order is not as important a feature of syntactic structure as might be imagined. It is certainly a much less important feature in Russian, thus lending further support to the critique developed by Bever et al. There must be something in LAD, the built-in "language acquisition device" discussed by McNeill (in press) and others, which favors beginning language with ordered sequences of unmarked classes, regardless of the degree of correspondence of such a system with the input language.[2]

Later Syntax

I have not yet examined Gvozdev's work to determine what happens to syntactic patterns after this early level; his classification of sentence types is not always the most useful, and extensive effort would be required--and should be expended--to reorganize his data for other sorts of analysis. He contends that by age three almost all of the complex and complex-subordinate sentence types of adult Russian are present, and that the child knows all of the generic grammatical categories (case, gender, tense, and so on) and has a good idea of their meanings. No new uses of grammatical cases enter after 3,9. By contrast, the learning of morphology and morphophonemics goes on for very much longer. It takes until seven or eight to sort out all of the proper conjugational and declensional suffixes and categories, stress and sound alternations, and the like. The Russian child does not fully master his morphology until he is several years older than the age at which the American child is believed to have essentially completed his primary grammatical learning. In this sense, then, it may be more difficult to learn to speak one language natively than another--though the basic learning is accomplished very rapidly.

(This point cannot be properly evaluated, however, until we have more information about the grammar of English-speaking children between the ages of five and eight. Full mastery of the auxiliary system, the subjunctive, and quantifiers, for example, is quite late in American children. It is not yet possible to adequately compare the lateness of such accomplishments with the lateness of other sorts of accomplishments in Russian. The unanswered question is whether the speech of a Russian seven year old is heard as more deviant from adult speech than is the speech of an American seven year old.)

Morphology

Morphological markers enter when sentences increase from two to three or four words in length. All words are unmarked in Zhenya's speech until about 1,10, and then, in the one month between 1,11 and 2,0 there is a sudden emergence of contrasting morphological elements in various grammatical categories. In this one month, previously unmarked nouns are marked for: (1) number, (2) nominative, accusative, and genitive cases, and (3) past tense, and (4) present tense. Apparently once the principles of inflection and derivation are acquired-

or, at any rate, the principle of suffixing--the principle is immediately applied over a wide range of types.

There are many other examples of simultaneous emergence of a grammatical principle in several domains. For example, gender agreement appeared simultaneously both in regard to adjective-noun agreement and noun-past tense of verb agreement. When a new grammatical case enters, it serves several functions at once. A variety of prepositions enter within a short period, and are combined with nouns in various grammatical cases. One is struck by the rapidity with which a principle is suddenly applied to an entire domain--and to the correct domain.

(Note, by the way, that the Russian child has no apparent difficulty in discovering morpheme boundaries. From the very beginning of inflections one sees a free use of word stems combined with a huge variety of bound morphemes. The word stem is clearly a psychologically real unit.)

Overregularizations are rampant in the child's learning of Russian morphology--small wonder, with the great variety of inflectional categories, and with the additional great variety of forms within each category, determined on the basis of both sound and grammatical relations. For example, not only must the child learn an instrumental case ending for each masculine, feminine, and neuter singular and plural noun and adjective, but within each of these sub-categories there are several different phonologically conditioned suffixes. The child's solution is to seize upon one suffix at first--probably the most frequent and/or most clearly marked acoustically--and use it for every instance of that particular grammatica category. For example, Gvozdev's son Zhenya at first used the suffix -*om* for all singular noun instrumental endings, although this suffix is used only for masculine and neuter singular nouns. This suffix, however, has only one other function--a masculine and neuter prepositional case ending for adjectives. The corresponding dominant feminine singular noun instrumental ending, -*oi*, on the other hand, serves a variety of functions, being an adjectival suffix for four cases in the feminine and one in the masculine. Thus, although feminine nouns are more frequent in Russian child speech, Zhenya initially used the suffix of fewer meanings-- -*om*-- for all instances of the instrumental case. This clarifies

Gvozdev's statement that grammatical are acquired earlier than morphological details. The child already possesses the category of instrumental case--and marks it accordingly--but it will take several years, perhaps, before he learns to correctly mark every instance of the instrumental in accordance with gender and with morphophonemic principles.

Large-scale research with preschoolers (Zakharova, 1958) has similarly revealed early stereotyped case endings for each case in a child's repertoire. Like Zhenya, Zakharova's subjects used the suffix -om as a universal instrumental, and -u as a universal accusative. These endings are of high frequency, clearly marked accoustically in adult speech, and limited in the number of functions they perform.

Zakharova also found that, as gender comes to be more important in classifying nouns, other additional endings for each case enter. They do not, however, peacefully coexist with the already established endings. When a child learns, for example, that -oi--the feminine noun singular instrumental ending--can also serve as a noun instrumental ending, he abandons the masculine and neuter instrumental, -om, which he has been using, and for a while uses -oi as a universal instrumental. Only later does -om re-enter to assume its place in standard Russian. Practice clearly does not insure the survival of a form in child speech--regardless of whether or not that form corresponds to adult usage (and, presumably, regardless of whether or not its usage by the child is "reinforced" by adults). (This is very similar to the development of the past tense in English, in which irregular strong forms, like did, are at first used correctly, only to be later driven out by overgeneralizations from the regular weak forms, giving rise to transitory though persistent forms like doed.) Popova (1958) presents additional evidence of ontogenetic replacement of one suffix by another, finding that very young children overgeneralize the feminine past tense of the verb, and that older children overgeneralize the masculine.

As noted above, full mastery of the morphological system comes relatively late in Russian-speaking children. The distinction between mass and count nouns is not stabilized until age eight; the distinction between animate and inanimate nouns in the accusative is mastered only at four; gender agreement between nouns and verbs in the past comes at three, although agreement of number and person come a year earlier; declension of masculine and feminine nouns ending in palatalized consonants is not mastered until six or seven. Soviet psycholinguists interpret the order and rate of acquisition of morphological classes in terms of the relative semantic or conceptual difficulty of various classification criteria

One line of evidence in this argument is the observation that lexical items referring to certain semantic categories appear at the same time as those categories become morphologically marked. For example, at 1,10, in Zhenya's speech, one finds the first use of the word mnogo (much, many) at the same time as the singular-plural distinction in noun markings. The words "right away" and "soon" enter at the same time as the future tense. And so on.

An attempt is made to set up the following order of acquisition of morphological classes in reference to their meanings:

(1) Those classes whose reference is clearly concrete emerge first. The first morphological distinction is number, at 1,10, followed shortly by diminuitive suffixing of nouns. The imperative, with its immediate, expressive character, also appears very early.

(2) Classes based on relational semantic criteria--cases, tenses, and persons of the verb--emerge later than those with concrete reference.

(3) The conditional is very late, not being used until 2,10, though its grammatical structure is exceedingly simple. Conditional subordinate clauses are also later, emerging at about 2,8. In both cases, it seems to be the semantic or conceptual, and not the grammatical aspect which is difficult for the child.

(4) Noun endings indicating abstract categories of quality and action continue to be added until as late as seven. The only derivational noun suffixes learned before three are those of clearly concrete or emotive reference--diminuitive and augmentative, endearing and pejorative.

(5) Grammatical gender is responsible for what is perhaps the most difficult and drawn-out linguistic learning of the Russian-speaking child, although it is almost always unequivocally marked phonetically. This is a category almost entirely lacking in semantic correlates, and apparently such correlates are an important aid in learning form-class distinctions. At first the child uses the feminine past tense ending for almost all nouns, regardless of their gender markings--even if he knows they are semantically masculine (e.g. papa). Later the child will use the masculine past tense for many nouns which are semantically feminine. The verb inflection is simply not treated as having semantic content. Likewise, the child will first use one stereotyped case ending for all nouns in that case, regardless of their gender (even if can correctly identify gender-class membership on the basis of pronoun substitution and adjective agreement).

The semantic and conceptual aspects of grammatical classes thus clearly play an important role in determining the order of their development and subdivision.

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(More detailed discussion of Soviet psycholinguistics and Soviet child language can be found in Slobin [in press] and references cited therein.)

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Footnotes

1. The transliteration is a very rough approximation to the sounds of Russian, and does not always match the Russian orthography.

2. It may well be that order is important in the "base structure" of Russian, thus supporting McNeill's proposal (in press) that children "talk base strings directly." The most economical representation of an inflected language like Russian would order the language in the underlying representation. Inflections could then be added to the characteristic positions of parts of speech, and an additional rule or rules would then re-order this string. All of the world's languages make use of order in their grammatical structure, but not all languages have morphological systems. It would be reasonable, then, for LAD to assume the language to be ordered, to adopt a given order as a first guess, and later learn that it can be changed. This interpretation, of course, minimizes the contribution of the linguistic input, suggesting that it is more important in providing tests for hypotheses about the organization of language than it is in acting as an observation base for inference.

The Development of Interrogative Structures in Children's Speech

Ursula Bellugi

> "The more we learn about language,
> the harder language learning looks."
>
> Jerry A. Fodor:
> How to Learn to Talk; Some Simple Ways.

Until recently, psychologists ignored the problem of development of language in children except to count parts of speech as if they were the same as categories in adult speech and the like. Then the development of American structural linguistics as a discipline parallel to American behaviorism began a new interest in language acquisition. Some research centers were stimulated to begin studies of child language, in much the same way as anthropological linguists studied foreign tongues in the field. Psychologists began looking at child language as if it were an exotic unknown language to be described by techniques which had been developed by structural linguists. In the midst of this new confrontation of psychology and linguistics, the transformational generative theory of grammar arose to suggest dramatic and basic changes in the entire concept of linguistics, the theory of language, and the capacity for language acquisition.

This theory of language holds serious promise for an understanding of the structure of language, the relations between sentences, and an elucidation of the creative aspects of language; that is, the fact of novelty of utterances. And it entailed a serious recasting of some basic notions in psychology, at least to the extent that it is now clear that psychological theory was inadequate to account for language acquisition.

Transformational grammar, then, gives us a powerful tool in working out the structure of language and in considering afresh the problem of language acquisition. My approach in studying the development of interrogative and negative structures in children's language has been to work through serious formulations of those portions of English grammar by classical grammarians and by transformational grammarians.

A very thorough and insightful classical work has been written by the Danish linguist, Otto Jespersen, on Negation in English (1917), and a very elegant exposition of negatives in terms of transformational theory is presented by Edward Klima in an article which appears in Fodor and Katz, The Structure of Language (1964). We do not yet have the equivalent work in interrogative structures, although there are a number of partial and preliminary formulations. Chomsky (1962), Katz and Postal (1964), Lees (1960), and Klima (personal communication) all have devoted some attention to interrogatives, but all of these leave many questions about questions unresolved. However, they point in interesting and provocative directions in the analysis of questions in English.

Interrogatives in English

The result of these investigations show several facts of importance. I'd like to ask that we consider sentences as strings of symbols (you might try replacing the words with X - Y - Z) upon which certain operations can be performed. For example, you can permute or change around two of the terms in the string to get Y - X - Z. The resulting string, after rearrangement, will be related in ways we can define to the original string. In order to make this easier, we can use jabberwocky or nonsense instead of words, and try to get at what makes a string of symbols felt to be a question.

Suppose you have the sentence

THE MOG WIBBLED THE GOMP.

Notice first that you can be quite sure of the elements in the sentence, even though the main words are nonsense. THE MOG is obviously the subject of the sentence, WIBBLED is a transitive verb in the past tense, and THE GOMP is the object. I'd like to ask you to make up some questions using the information from that sentence; that is, transform it into several different questions. We would find a range of results like the following:

DID THE MOG WIBBLE THE GOMP?
WHY DID THE MOG WIBBLE THE GOMP?
CAN THE MOG WIBBLE THE GOMP?
WHAT WIBBLED THE GOMP?
WHAT DID THE MOG WIBBLE?
etc.

I don't think that you would have any difficulty in constructing questions like these from source sentence. An analysis of the operations that are involved in transforming the original sentence to any of these questions is exactly the point of the first part of this paper.

Notice that there are other variations which seem to have a question aspect:

THE MOG WIBBLED WHAT?
THE MOG WIBBLED THE GOMP?
THE MOG CAN WIBBLE THE GOMP, CAN'T HE?

but not:

WHAT THE MOG WIBBLED WAS GOMP.

Notice that the last sentence has some of the characteristics of questions: an interrogative word WHAT is at the beginning of the utterance; the intonation pattern is the same as a WHAT question; but it is not regarded as a question. This is another fact about patterns of the language which our analysis must take into account.

To lead those of you who have had no contact with linguistic analysis down these pathways may be indeed like wandering in an underground labyrinth, or worse yet, some level of detail that is irrelevant to language functions as you have so far considered them. But I would like to suggest that you follow the first part of this discussion by leaving aside your notions of grammar from grammar school, your involvement and facility with language, and moving back one step. Consider a sentence of a language as a set of symbols arranged in a string with a certain definable structure, and consider that operations can be performed on these strings that transform them to other related structures; for example, active to passive, affirmative to negative, declarative to interrogative.

These operations are entirely within the capacity of any speaker of a language and are capacities which are acquired by children in their acquisition of syntax, and therefore are germane to our consideration of the development of language functions. It is really only when we begin to look at _what_ is learned in some depth that we can begin to ask about _how_ language is learned.

If we take a simple sentence and some questions formed from it, we can examine in some detail what happens to change one into the others. Take a sentence like the following:

CHAGALL MADE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM.

We want to analyze several questions which can be constructed from the above string of words.

DID CHAGALL MAKE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM?
DIDN'T CHAGALL MAKE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM?
WHAT DID CHAGALL MAKE FOR THE CITY OF JERUSALEM?
WHO MADE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM?
CHAGALL MADE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM, DIDN'T HE?

A. Intonation Contour

Some of the above questions have intonation patterns that are different from the rest, namely, the ones which do not have an interrogative wh word. These are generally called yes/no questions because they can be answered by yes or no only, while the wh word questions require a different kind of response.

Try replacing the syllables of the string with da and listen for the changes in pitch level.

A declarative:

CHAGALL MADE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM.

Da-da da da da da-da da da da-da da da-da-da-da.

This is usually described by linguists as a 2-3-1 pitch contour, and the point here is that the pitch goes down at the end of the sentence.

A wh word interrogative:

WHAT DID CHAGALL MAKE FOR THE CITY OF JERUSALEM?

Da da da-da da da da da-da da da-da-da-da?

While the pitch level change is not the same as the declarative intonation, again the pitch level goes down at the end and fades.

A yes/no interrogative:

DID CHAGALL MAKE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM?

Da da-da da da da da-da da da da-da da da-da-da-da?

In this case, the pitch level goes up and stays up, ending on a higher level than the one on which the sentence started.

One operation involved in the construction of yes/no questions, then, is a change in intonation contour so that the pitch level rises and stays high. This is generally denoted as 2-3-3 and is a signal for questions. Notice that this is one way of converting a sentence to a question without performing any other operations.

Contrast:

The boy played with his dog?
The boy played with his dog.

The rising pitch level also applies to the tag question, can he? or didn't she?

B. Auxiliary Inversion and Negation

Examining again the declarative sentence and the questions formed from it, we can begin to look at the auxiliary verb in each case.

In the declarative sentence, there is a main verb which is in the past tense (Make + past tense = made) and no auxiliary verb. In all but the wh-subject question, there is an auxiliary verb, which is a form of do. Looking more closely, we notice that it is the past tense form of do and that the main verb has lost the past tense marking. We can consider that the past tense marker has moved from the main verb and been incorporated into the auxiliary verb.

Looking at a sentence which already has a modal auxiliary, we can begin then to see the function of the do auxiliary in the above set of materials.

I can go out.
Can I go out?
Where can I go?

Notice that the auxiliary of the verb has switched places with the nounphrase in both forms of question. It looks like, in order to construct most questions, the nounphrase and the auxiliary verb of a sentence have to be interchanged. That this is an intrinsic part of questioning in English is suggested by the fact that this word order sounds like a question even without the rising intonation and without a wh word. Can I go out, while not strictly a question, seems to suggest interrogation. So we have a second operation which defines questions in English, and that is that the auxiliary verb moves before the nounphrase of a sentence under interrogation.

The appropriate form of the auxiliary *do* is supplied whenever there is no auxiliary in the corresponding declarative sentence. We have noticed that verbs without auxiliaries can be inflected for tense, i.e., either the past tense or the marker for the present indicative tense, which shows up in the third person singular form only.

We walk.
I walk.
They walk.

He walk*ed*.
He walk*s*.

Under questioning, when the auxiliary *do* is supplied, *do* carries the tense markings rather than the main verb. Thus, the above set becomes:

Do we walk...?
Do I walk...?
Do they walk...?

Did we walk...?
Does he walk...?

Negation is most frequently accomplished in English by attaching a negative element (*not* or its contraction *n't*) to the auxiliary of the verbphrase. This is carried over with the auxiliary in inversion for questions. As we have seen, not all sentences have auxiliary verbs, and one is required for both questioning and negation. If there is no auxiliary an appropriate form of *do* is supplied to carry the negation. Thus we find:

He can play soccer.
Can he play soccer?
What can he play?
He walks.
Does he walk?

He can't play soccer.
Can't he play soccer?
Why can't he play soccer?
He doesn't walk.
Doesn't he walk?

This seems rather a complex story, but can be summarized as follows: In almost all questions, the auxiliary component and the noun-phrase subject are interchanged, or inverted. If there is no auxiliary, the appropriate form of *do* is supplied which carries the tense markings with it in inversion. For sentence negation, the negative element *not* or *n't* is connected with the auxiliary and moves with it before the nounphrase.

C. Wh Word Placement - Object Questions

Contrast the following:

CHAGALL MADE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM.
WHAT DID CHAGALL MAKE FOR THE CITY OF JERUSALEM?

First of all, something is missing from the array of words which comprise the original sentence, and that is the object of the verb: *stained glass windows*. Instead we find an interrogative word WHAT at the beginning of the question. If you think about it, it is clear that it is the object nounphrase which is being questioned, and to which the what refers. Rearrange the string in another way which means the same thing, and the operation involved may become clearer:

CHAGALL MADE WHAT FOR THE CITY OF JERUSALEM?

The question word what refers to the object position of the sentence, and is moved to the beginning of the string to produce the question, WHAT DID CHAGALL MAKE FOR THE CITY OF JERUSALEM? Notice that the auxiliary inversion operation has already applied.

D. Wh Word Placement - Subject Questions

Consider in addition to the above two utterances:

WHO MADE STAINED GLASS WINDOWS FOR THE CITY OF JERUSALEM?

In this case, the subject has been replaced by the wh word, and no movement is necessary since the positon replaced is first in the sentence. In addition, these questions differ from object questions in that no inversion takes place, and do is not supplied when no auxiliary is present in the sentence.

The main operation we have suggested here is: Any nounphrase position in a simple sentence can be questioned. If an object nounphrase is questioned, the element that is being questioned is replaced by a wh interrogative word and is moved to the front of the question, after the operation of auxiliary inversion. If a subject nounphrase is questioned, the wh word replaces the subject position and the word order remains the same.

To emphasize the point, we can return to nonsense syllables. Suppose we use the same nounphrase in both positions:

WHAT CAN WIBBLE THE BIK?
WHAT CAN THE BIK WIBBLE?

We still can know that in the first case, the bik is the object nounphrase and it is the subject which is being questioned; and in the second case, the bik is the subject nounphrase and the object is being questioned.

E. Tag Questions

Tag questions like:

He can go, can't he?
She didn't say, did she?

are formed in a regular fashion. After a comma which is marked by a continuing pitch level in speech, the auxiliary of the sentence is repeated (with a negative element if the sentence is positive, without negation if the sentence is negative), and the subject nounphrase is pronominalized. The tag carries a rising intonation contour like the question form. If there is no auxiliary, do is supplied and carries the tense attachments as in other questions.

He goes fast, doesn't he?
The girl didn't go home yet, did she?

We have discussed several operations which seem to be involved with interrogation in English in some intimate way. These may seem to be fine points and a great deal of detail, but if one considers that the appropriate use of these few operations will enable one to transform an infinite set of sentences into an infinite set of questions, the power of this set of tools becomes more obvious. The fact that I could give you any simple sentence in the English language and you could construct a yes/no or wh interrogative from it (presumably by using these operations) means that it is a capacity about language to be considered when we discuss language acquisition. The child by the age of four or so can accomplish the same feat. The stages by which he arrives at this ability are the concern of the last part of this paper.

A Note on Historical Development of Interrogatives

There has been considerable shift in the use of the auxiliary system from old English to modern English, as this set of questions taken from the plays of Shakespeare will reveal:

Yes/No Questions

Shrug'st thou, malice?
Heard you this, Gonzalo?
Seest thou here?
Well, then, go you into hell?
Look you for any other issue?
See you where Benedict hath hid himself?
Sits the wind in that corner?

Wh Word Questions

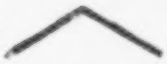
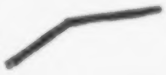
What says he to your daughter?
Which way looks he?
How came you to this?
How know you he loves her?
Why speaks my father so ungently?
How say you?
Where had he wine?
Wherefore weep you?

In modern English, the auxiliary system bears a heavy grammatical burden, in that four universal grammatical functions are intimately connected with the auxiliary verbs (Twaddell, 1963). We are concerned here with three of these: the auxiliary carries the *n't* of negation; it is inverted with the subject nounphrase in interrogation; it is involved with the formation of *tag* questions. In all these cases, modern English requires the insertion of an auxiliary (*do*) wherever one is not present in the original sentence. This last is fairly recent development in English. At the time when Shakespeare was writing his plays, it was an optional rule of the grammar. One frequently found questions without an auxiliary, and in these cases, the main verb was inverted with the subject nounphrase to signal interrogation

It is the absence of *do* which gives the special character to the set of questions quoted above, and the fact that the main verb has been inverted with the subject instead. For example, *Seest thou here*? today becomes *Do you see here*? and *What says he to your daughter*? changes to *What does he say to your daughter?*

It is interesting to notice that in the three children we studies there was no tendency to repeat this historical development, and inversion in questions does not appear until after the auxiliary system develops in the children's speech.

Summary of Interrogatives in English

Rising Intonation Contour: The normal speaking sentence pitch pattern is shaped like this:  A question intonation pitch pattern rises somewhere toward the end of the utterance and stays up, like this:  This intonation contour converts any sentence into a question, occurs with *tag* questions, and with *yes/no* questions (but not *wh* word questions). Thus: The boy went out? Did the boy go out? The boy went out, didn't he?

Auxiliary Inversion and Negation: In almost all questions, the auxiliary component and the nounphrase subject are interchanged (the exception: subject nounphrase questions). This operation alone converts a string into a questionlike utterance, even without rising intonation. The yes/no questions have additionally operation I, and the wh word questions have an interrogative word as well as auxiliary inversion.

The negative element (n't or not) is connected to the auxiliary and moves with it.

The boy can go there.	Can the boy go there? Where can the boy go? Why can't the boy go there?

Wh Word Placement: An interrogative word (what, who, why, where, when, etc.) may be considered as replacing some missing element in the sentence, for example, the subject or object nounphrase. This interrogative word marker can then be thought of as moving to the front of the question, after operation has applied.

John hit what?	What did John hit?
The boy can play with what?	What can the boy play with?

Subject questions simply replace the subject position nounphrase with the wh interrogative word, and no auxiliary need be supplied.

Formation of Tag Questions: Tag questions are formed by repeating the auxiliary of the sentence (or adding do if there is none); adding a negative to the auxiliary if the main sentence is non-negative, but leaving the negation off if the main sentence is negative; and a pronoun or pronominalized subject.

The flowers can stay there, can + n't - they - ?
The oranges aren't in the bowl, are - they - ?

Description of Data[1]

For more than four years, our research group (Prof. Roger Brown and his associates[2]) has been studying language acquisition. We have as data for this research a development study of the language of three children. We collected two hours of speech every two weeks in a natural setting; that is, recordings of conversations between mother and child in the home. We have supplemented this data by performing small experiments to begin investigatio of the child's grammatical comprehension and competence.

The children we have been studying are a special sample; Adam and Eve were chosen because they produced a high rate of speech that was largely intelligible. The fathers of both children were graduate students at Harvard, and the children were only children when the study began. We have added a third subject to our small sample whom we are calling Sarah. She is the daughter of parents who have not completed high school, and her language environment contrasts with that of the other two. We looked for a situation in which the mother did not expand the child's speech as often as did the mothers of Adam and Eve.

Obviously the sample seems exceedingly small for generalizations about the development of language in children. Comparison of our data with data from other research centers[3] and with studies made by Brown and Fraser (1964) on a dozen children show that developmental milestones seem to be remarkably constant across children, and therefore the intensive study of a small sample seems highly appropriate.

With these three children, each child was followed by a different investigator; the families were totally unacquainted and independent of one another; each child heard a different set of sentences as "input"; and yet the language milestones as we shall see are constant.

The children were beginning to string words together in two- and three-word utterances when we began the study. Their speech was characterized by a lack of inflections and an absence of functors and was largely telegraphic, as described in an earlier paper (Brown and Bellugi, 1964). When we began the study, Eve was 18 months old, Adam 26 months old, and Sarah 27 months old; however, all three were at approximately the same stage of language development.

Stages	Eve	Adam	Sarah
I	18	28	27
II	22	32	32
III	25	38	38

Table 1A. Ages in Months at First Sample Chosen for Inclusion in Study.

Stages	Eve	Adam	Sarah
I	1.8	2.0	1.8
II	2.9	2.5	2.3
III	3.6	3.6	3.4

Table 1B. Mean Utterance Length for all Utterances in Samples Combined to Make up Stages.

For each child, then, we have two to four sessions of the speech of the mother and the child per month as data. These sessions were tape recorded and later transcribed together with a written record made at the time of the recording which includes some aspects of the situation which relate to the meaning of the interchange. We have analyzed the mothers' speech as well as the children's so that we have some knowledge of the speech sample each child has heard and some basis for comparisons between mothers of mother-to-child speech.

In general, we have found that the frequency profiles of the mothers are very similar for sentence types. The correlations range from.65 to.90. We found that the correlations between two profiles on one mother taken in different weeks were as high. Sentence types seem to have a relatively stable frequency over numbers of samples in the speech of the mothers. One interpretation of these data is that there is a standard mother-to-child language when viewed in terms of sentence types.[4]

In order to describe stages in development, I have pooled the data of all three children to pick sample questions for discussion and analysis. In the appendix, however, one can find the samples separated out for each child. I have used some rough indicators to pick comparative samples--the best general indicator of language development still seems to be mean utterance length. I picked two end points in terms of mean utterance length: the first stage is from the first month of the study for each child; the last is from the month in which the mean utterance length consistently went above 4.0 for each of the three children. Then I picked four points in between dividing the span of time covered into equal segments. This gave six samples for each child. Because this story is far too long for a conference report, I have pooled these into three stages combining two at a time, but I shall report on the data more fully elsewhere, and the data warrant further divisions than I have made here.

Stage 1.

The structure of the children's sentences at stage 1 has been described often in our past papers. The sentences average only two morphemes in length, and consist most often of unmarked nouns and verbs. Notably missing are inflections, auxiliaries, articles, determiner quantifiers, even most pronouns. There is very little structure to be described, but it is worth noting that even at this early stage, the child has means of expressing basic sentence functions, as declarative, negative, imperative, interrogative. The sentences include: *Papa go*. *Baby up*! *No Mommy eat*. *Mommy read*. *No sit there*. *No a boy bed*. etc.

There are interrogatives, with and without a *wh* word. These are some of the questions we want to describe:

Fraser water?
Mommy eggnog?
See hole?
I ride train?
Have some?
Sit chair?
No ear?

Who that?
Why?
Why not?
What's that?
What doing?
What cowboy doing?

Where Ann pencil?
Where Mama boot?
Where kitty?
Where my milk go?
Where horse go?

Structure of Questions: It seems that almost any utterance can be transformed into a question by changing the intonation pattern to 2-3-3. Since there is so little structure to these sentences, it is not possible to define any constraints on questions. There are none of the identifying characteristics of *yes/no* questions, since there are no auxiliaries, no subject-verb inversion, and relatively little tense marking.

The *wh* questions can be described as a list which includes only a few routines that vary little across the three children. The most common questions are some version of *What's that*? and *Where Nounphrase (go)*? and *What (NP) doing*? It is not even clear that there is always a structural distinction between *wh* and *yes/no* questions at this stage. One child has both *Where boy go*? and *Boy go?*

The only one of the operations which we have described for adult English which is present at this stage is the rising intonation contour that marks *yes/no* questions. It seems that any utterance can be converted into a question by changing the intonation pattern.

S - Q

where Q is a rising intonation contour over the sentence.

There are a few negative questions. Negative at this stage is not in position to be connected with the auxiliary of a sentence and there are no auxiliaries. It is a marker which generally precedes a sentence and occasionally follows. The basic rule for negative at this stage is:

Neg - S

where Neg is no or not. Negative questions are formed by pronouncing this with a rising intonation contour:

Neg - S - Q

There are not tag questions, nothing like wh questions which question subject or object position, except for the few routines described. This stage can best be characterized as the stage when the child has a question marker (a rising intonation contour) and a few routine wh questions. Two of these routines act as frames for the noun or nounphrase of an utterance.

Where NP (go)?

Where kitty?
Where string?
Where Ann pencil?
Where Mama boot?
Where ball go?
Where Adam go?
Where Papa go?
Where my milk go?

What (NP) doing?

What cowboy doing?
What paper clip doing?
What doing?
What you doing?

Comprehension of Questions: Our data consists of recorded conversations between mother and child, and it is possible to gain some insight into the child's comprehension of grammatical constructions by examining his response to certain types of questions that the mother asks. An example would be the what-object questions. In order to give an appropriate response, we can presume that the child must understand that the object is being questioned; that the wh interrogative word refers to the object of the verb. If we take the set of what-object questions which the mother asks in the course of the samples of speech, we find that generally the child does not respond, or responds inappropriately.

For example:

Mother	Child
Well, what did you hit?	Hit.
What did you do?	Head.
What are you writing?	Arm.
What do you want me to do with his shoe?	Cromer shoe.
What are you doing?	No.

It seems clear, then, that at this stage the child is not producing questions that even superficially resemble *what*-object questions, and that he does not understand this construction when he hears it.

Stage 2.

There is considerable development in the structure of the sentence since stage 1. Notably, pronouns have developed, articles and modifiers are often present, some inflections (present progressive and plurals, irregular past forms), and the verbphrase may include a prepositional phrase or a preverb. There are still no auxiliaries, but two negative preverb forms (*don't* and *can't*) which are prior to development of an auxiliary system. There are no indefinite pronouns and no clauses or other signs of embedding in the grammar.

There are some minor developments in interrogatives. Some of the questions we want to consider are:

See my doggie?
Dat black too?
Mom pinch finger?
You want eat?
I hab it?

Where put him on a chair?
Where my mitten?
Where baby Sarah rattle?
Where this goes?
Where me sleep?

What getting?
What book name?
What happen me?
What in there?
What methink?

Who bought that?
Who is it?
Who brought him home?
Who making that noise?

Why?
Why you smiling?
Why you waking me up?
Why need them more?

Why not?
Why not he eat?
Why not me sleeping?
Why not...cracker can't talk?
Why not...me can't dance?

You can't fix it?
This can't write a flower?

Structure of Questions: Yes/no questions are signalled by questions intonation only, as in stage 1, for two of the three children. Since there are no auxiliaries, there is no auxiliary inversion. But there is no subject inversion either, as in the Old English form. One of the three children has an additional rule for forming yes/no questions, which is worth noting since it is so clearly a precursor of the well-formed yes/no question which will appear in the next stage. Adam produces numerous examples in each sample of these:

D'you want it's turn?
D'you want me...should be careful?
D'you want me tie that?
D'you want me get it?
D'you want me have birthday?
D'you want me drink it?
D(you) want me that go?

The usual form is: D'you want (pro) VP? and after several weeks the D'you want has become fused into a single word pronunciation. This seems clearly a question marker for yes/no questions. It operates almost like the pivot plus open class constructions of the very earliest stage. This question marker (D'you want- pro) operates as a yes/no question introducer, and can be associated with Adam's range of verbphrases (and an occasional sentence: D'you want me that go?). We will see that this invariant form has dropped out in the next stage when the auxiliary system is available and yes/no questions are well-formed.

The yes/no questions with a negative preverb are formed by the operation of the question intonation only. One finds You can't fix it? rather than the well-formed Can't you fix it? There are no tag questions at this stage.

In the wh questions, all wh interrogative words are in initial position; the auxiliaries are missing, and there is no subject-verb inversion. There is very little evidence for replacement of a missing element or constituent of a sentence by the wh word and the preposing of this wh interrogative at the front of the question (operation C of the adult rules) which we will find in the next stage. Instead it seems simplest to consider the wh interrogative word as a question introducer which does not yet replace anything in the underlying form. Questions like the following make it difficult to distinguish subject and object questions:

3

Who bought that?
What getting?
Why need them more?
Where put him on a chair?

and suggest the analysis of the *wh* question at this stage as an interrogative *wh* word followed by VP or S.

Further evidence in this direction is the set of *Why not* questions. We find *Why not he eat*? and *Why not...me can't dance*? in Adam's speech. The simplest description of this set is

Why not - S

where S may contain a negative preverb. At no place in the children's grammar in this stage do we find multiple negation, and this form also drops out by the next stage.

The sequences we have suggested for questions, then, and the operations which seem to be involved at stage 2 are:

A. Any sentence can be converted into a *yes/no* question by the application of Q, where Q is question intonation.

S - Q

One of the three children has the additional *yes/no* question marker:

D'you want - (pro) - VP - Q

B. There is no evidence yet for auxiliary placement and inversion. We find that sentences can have a negative preverb which is carried over into the question form but does not function as an auxiliary. Adam's invariant formula (*D'you want*) is not an auxiliary operation. There is no verb subject inversion.

S - Q

where S can have a negative preverb, $\text{NP} - \left\{\begin{matrix}\underline{\text{can't}} \\ \underline{\text{don't}}\end{matrix}\right\} - \text{VP}.$

C. The *wh* interrogative word in this stage seems to function largely as a question introducer at the beginning of the question, rather than a replacement for a missing constituent in the sentence which has been moved to the front of the question. The evidence is not clear on this point, but is insufficient to credit the child with this operation, while it clearly is effective in the stage which follows this.

Wh - S

The following restrictions are loosely suggested for the distribution of S:

Who - VP
Why - S
Why not - S (where S may contain negative preverb)
Where - S
What - S

One other point that requires notice is that the first person singular reference in these questions is often pronounced *me* (or *my*) instead of *I*. This also drops out by the next stage.

D. There are no *tag* questions yet. One sometimes finds *You did it, huh*? or *all right*? but no *You did it, didn't you*?

Comprehension of Questions: We find by stage 2 that there are appropriate answers to most of the common questions. Looking specifically at the child's answers to *what* object questions we find a change since stage 1 when the child either did not respond or gave an inappropriate response. At this stage the balance has shifted, and all three children are giving largely appropriate answers to *what* object questions. The responses reflect that the child understands that the object of a verb or preposition is being questioned.

Mother	Child
What d'you hear?	Hear a duck.
What do you need?	Need some chocolate.
What else did you see?	See bridge.
What d'you have?	Have sugar.
Who do you love?	Mommy, you. I love fishie too.

There seems evidence of comprehension of an operation involved in constructing questions, but little evidence that the child uses this operation in constructing his own questions until the next stage.

Stage 3.

Between the last stage and this there is an impressive and sweeping set of developments in the children's grammar. For one thing, the auxiliary system has by stage 3 appeared in full flower in the children's speech. One finds almost at the same time auxiliary verbs appearing in declarative sentences, in negation, in *yes/no* questions, and sometimes in *wh* questions, although often in non-inverted form. The only preparatory indicators in the previous stage were one child's *D'you want* (pro) question marker, and the placement of the negative preverbs *don't* and *can't* between subject and verb in all three children.

In addition we find the full array of noun and verb inflections appearing, including the third person singular present indicative, and the regular past. We find also that the sentences are no longer limited to simple English sentences. There has been some development in complexity, and we find clauses and other embeddings present for the first time: *You have two things that turn around*. *I told you I know how to put the train together*. *I gon' get my chopper for chopping down cows and trees*. *They don't turn when I get on the floor.* *Let's go upstairs and take it from him because it's mine*.

These and other changes in the grammar are clearly mirrored in the interrogative system, which is strikingly different from stage 2. Consider the following questions:

Does the kitty stands up?
Does lions walk?
Is Mommy talking to...Robin's grandmother?
Did I saw (that) in my book?
Oh, did I caught it?
Are you going to make it with me?
Do I look like a little baby?
Will you help me?
Can I have a piece of paper?

Where small trailer he should pull?
Where the other Joe will drive?
Where I should put it when I make it up?
Where's his other eye?
Where my spoon goed?

What I did yesterday?
What he can ride in?
What you had?
What you writing about?
What did you doed?
What are you doing to read for me, Fraser?
Sue, what you have in you mouth?

What lives in that house, Mommy?
Who took them all down?
Who took this off?
Who lost it?

Why the Christmas tree going?
Why he don't know how to pretend?
Why the kitty can't stand up?
Why Paul caught it?

Which way they should go?

How he can be a doctor?
How they can't talk?
How I push it back?
How that opened?

Can't it be a bigger truck?
Can't you work this thing?
Can't you get it?

Structure of Questions:

A. We find the question intonation operating over the *yes/no* questions as in previous stages, but now it involves the auxiliary system as well.

B. For the first time in the developmental data we have investigated, the auxiliary system is present and functioning in full. Auxiliary verbs have appeared in the children's declarative sentences as well as in negation and questions. The auxiliary verb is appropriately placed in declarative sentences, and the negative element *n't* is added for negation. Now almost all *yes/no* questions have an auxiliary (or some form of *do*) and the auxiliary and nounphrase are inverted. Notice the delicate tuning and agreements involved i

Is Mommy talking...
Did I hit...
Do you take...
Will you read...
Am I silly?
Are we talking...
Can we be...
Will we have...

Do you get...
Can I have...
Can't you work...
Are you tired...
May I whistle...
Did you drink...
Is Paul gonto be...
Is Fraser having...

And notice the following overextensions which we will consider:

Did I caught it?
Did I saw (that) in my book?
Did I lost one?
Did you broke that part?

Does the car carrier carries that?
Does the kitty stands up?

Does lions walk?
Does turtles crawl?

Some complications arise in tense marking when the dummy auxiliary *do* is supplied, but the operation of inversion of nounphrase subject and auxiliary verb seems clear. The form for a *yes/no* question, then, is:

Aux - NP - VP - Q

and the Aux component can have an optional negative attachment:

Aux + *n't* - NP - VP - Q

The auxiliary story with respect to the *wh* questions is different. When the auxiliarie are present (modals and *be*) they are often not inverted:

What the words are doing?
What he can ride in?
What I can put them in?
Where small trailer he should pull?
Where the man's truck should go?
Where I should put it when I make it up?
Why the kitty can't stand up?
Why he don't know how to pretend?

that is:

Wh - NP - Aux + (*n't*) - VP.

And if there is no auxiliary in the sentence, it is frequently not supplied, leaving the tense markers on the verb, and giving forms like:

What we saw?
What you took out?
What I did yesterday?
What you had?
How you opened it?
Where my spoon goed?
Where she went?
Why you caught it?

or:

Wh - NP - VP

The auxiliary form of *be* is also optional at this stage in *wh* questions:

What he writing?
What you writing about?
What Paul trying to see?
Where he gonna sit?
Why the Christmas tree going?

We have said that auxiliary inversion takes place in *yes/no* questions, and that the modal auxiliaries appear in *wh* questions, but generally are uninverted. At this stage, the children seem to place the auxiliaries appropriately for almost all questions, but invert them with the nounphrase only in *yes/no* questions. Note that *do* appears in *yes/no* questions, but seldom in *wh* questions.

There are a few negative yes/no questions, which are inverted, and a few negative wh questions, which are not, paralleling the above findings:

Can't it be a bigger truck?	Why the kitty can't stand up?

C. At this stage there is ample evidence for considering the interrogative word as a replacement for some missing element in the sentence (subject or object nounphrase) which is moved to the front of the set of elements to form a question. We have seen that in the previous stage, the children responded correctly to what object questions, but did not construct them productively. Wh subject and object questions are now differentiated

Notice that the string of words without the wh word is incomplete. It is no longer describable as S, but as NP - Aux - VP with some object NP missing from the VP. These cannot be considered as full sentences:

We saw...
You book out...
He can ride in...
You writing about...

There is another set of questions, the who questions, which have a nounphrase missing from the subject position in the sentence, if we subtract the interrogative word:

...lives in that house.
...took them all down.
...took this off.
...lost it.

D. There are no tag questions as yet. These appear in a later stage of the children's grammar.

Comprehension of Questions: The children understood and responded to what object questions in stage 2 already. In this data we find responses to more complex what object questions appropriate.

Mother	Child
What d'you need a rifle for?	I wanna shoot.
Then what will you do for milk?	I gonna buy some more cows.
What d'you think we should do?	I know what I should do; play with some more toys.

Summary of Stages of Interrogation

Stage 1

S	→	(Neg) - N (V) - (N)	Neg: *no* or *not*
Yes/No	→	S - Q	Q is rising intonation
Wh	→	Wh - (NP)	A list of routines. NP can vary in a few routines like, *Where NP (going)?*

Stage 2

S	→	NP - (neg prev) - VP	Neg preverb: *don't* or *can't*
Yes/No	→	S - Q	Q is rising intonation
Wh	→	Wh - S	

Stage 3

S	→	NP - Aux (+n't) - VP	Aux: modals, *do, be*
Yes/No	→	Aux - NP - VP - Q	Q is rising intonation, Aux has been moved.
Wh_o	→	$\left\{\begin{matrix}\text{Wh - NP - VP}\\ \text{Wh - S}\end{matrix}\right\}$	Object Q's. Sometimes *do* missing, and object NP is missing; aux not moved
Wh_s	→	Wh - VP	Subject Q's

We have looked closely at the set of operations involved in constructing English interrogatives, and examined three periods in the child's progression toward full mastery of these rules.

First, we have found that the milestones toward acquisition of language are constant across the three children. The three begin the process by stringing small sets of unmarked nouns and verbs, but develop similar ways of expressing basic sentence functions, like negative (neg + S) and interrogative (S + rising intonation, a few routine *wh* questions).

By the third stage of our investigation, the auxiliary verb system has come into play in full and across the board. That is, with all three children, auxiliaries were notably absent from speech in the early stages (say, when the mean morpheme per utterance length was below 3.4), and then within a relatively short period of time, auxiliaries abound in declarative sentences, negative sentences, yes/no interrogatives. Complex sentences or embedded sentences appear at this stage in the three children.

Second, we found that the children may have alternative pathways to the same goal that differ only at superficial levels. For example, in stage 2, Sarah and Eve both formed yes/no questions by rising intonation contour only. Adam formed yes/no questions in the same way, but had additionally a yes/no question marker, like a pivot construction, which consisted of an invariant form (D'you want [pronoun]), and he restated his verbphrases with that. On the surface, one might consider that Adam has developed the use of an auxiliary, the dummy do, and that this is inverted with the nounphrase to form quotations. But on consideration of all the evidence, this is a single invariant form (no other auxiliaries, no other inversions, no other forms of do even) and can best be described as an additional question marker, rather than an operation on sentences. Eve and Sarah use question intonation only to mark yes/no questions; Adam uses that plus his question marker.

Third, we have begun to look at the development of transformational operations, in children's language. It is not clear what the antecedents of transformational operations are, in stages 1 and 2. But the evidence is good for considering that the child performs certain operations on strings that are definable as transformational operations by stage 3. We find strong evidence for auxiliary inversion for interrogation, and for the do transformation, and good evidence for the replacement of some constituent in a string by a wh element, and the movement of that to the beginning of the question.

If we look closely at the data in stage 3, we find that the children can perform these three operations, but there seems to be a limit on the number of operations on one string at this period. That is, we find in yes/no questions, auxiliary inversion and the do transformation. At the same time, in wh questions, we find an absence of do and no inversion even when the auxiliary is present. There is clearly not an inability to perform each of these operations, but there may be some limit on the number which a child can do at one time. Typically, we find at this stage, _Can I go out_? _Why he can go there?_ and _What you took out_? A good deal more work will have to be done before there is solid evidence for this notion, but the productions of the three children we have studied at this stage seem highly suggestive.

Appendix

Sample Questions from each of Three Children
for Three Stages

Stage 1.

SARAH

Where kitty?
Where string?
Where?
Where go?
(What) (th)at?

Doggie gone?
Gone?
See hole?
Baby cry?
Kitty gone?
Boy?
My horsie 'nere?
I throw 'way?
No ear?

EVE

Why?
What doing, Mommy?
Where Papa go?
Where Ann pencil?
Where Mama boot?

More?
Mommy?
Papa get Anna pencil?
Tinker-toy doing there?
That?
Fraser water?
Taste it?
Kathy letter?
Anna table?

ADAM

What that?
Who that?
What happen?
What train?
Where ball go?
Where Dale go?
What cowboy doing?
What that paper clip doing?
What you doing no?
Where boot?
Why not?
What dat needle?

Sit chair?
No more milk?
Cromer, have some?
Ball go innere?
Mommy throw 'way?

Stage 2.

SARAH

Where Donna?
Where my mitten?
Where crayons?
Where you go?
Where put him on a chair?

Who got my baby?
Who is it?
Who rip dat?
Who bought dat?
Who milk is dat?
Who done this?
Why need them more?

What(s) that?
What color dis?
What's that on him mouth?

You want 'nere?
See my doggie?
Dat black too?
I had dinner?
You want bobby pins?
You ride once more?
Mommy, you want eat?
Rub it like dis?
It hurt?
He sit down?

EVE

Where Fraser elbow?
Where you coffee?
Where Sarah going?
Where baby Sarah rattle?

Why?
Why not?

What doing, Mama?
What that?
What Fraser you do?
What in there?
What the dollie have?
What Mom name?

Fraser, like more?
I hab it?
Let me eat it?
Mom, Mom pinch finger?
Mom little teeth?
These are my mitten?
You gonna watch me eat my lunch?
My tapioca cool?
Sue making more coffee for Fraser?

ADAM

Where Mommy?
Where my milk go?
Where dis goes?
Where other hammer go?
Where me sleep?
Where you put it?

Who dat making noise?
Who brought dis?
Who make dat?

Why not?
Why me say Shadow Gay all time?
Why me go?
Why me going?
Why you...waking me up?
Why it's all gone?
Why me get some chocolate?
Why not me sleeping?
Why not...cracker can't talk?
Why not...me can't dance?
Why not me break dat one?

What me think?
What you looking?
What you doing, Mommy?
What happen me?
What book name?
What getting?
What dose children doing?

Cromer want some...grapefruit?
Do like grapefruit?
Do want more coffee please?
Do want me dat go?
D'you want it's turn?
D'you want me...should be careful?
D'you want me tie that?
D'you want me get it?
Do want me drink it?
D'you want me have birthday?
D'you want drink it?
Do want he walk like dis?

Stage 3.

SARAH

Where he gonna sit?
Where's my other one?
Where is he now?
Where's his other eye?

Who lost it?
Who is it?

Why the Christmas tree going?
Why you...get to go down the stairs?
Why not?

Which one you want?

How you open it?
How you get them down?
How that opened?
How come you got lipstick on?
How many monkeys?
How you get them down?
What you had?
What my jingle bells doing up there?
What he writing?
What you writing about?

Do...you get two ice creams?
Can I have a piece of paper?
See my new bathrobe?
Do you want name?
Can we go up there?
Uh...will we have one?
Will I break it?
These for baby toys?
You got something in your mouth?
Can I do it?
Did you throw it away?
Can't you work this thing?
Will you help me?
Did I lost one?
Do I look like a little baby?
Is this pan cakes?
Can't you get it?
Is it on right?
You hear someping in here?
Are you going to make it with me?
Are you tired?

EVE

Where is a lady going?
Where Fraser and Cromer?
Where big round cook?
Where Papa go?
Where my spoon goed?

When Cromer and Fraser go home?

How I get in, silly?
How I push it back?

What are you going to have?
What are you going to read for me, Fraser?
What (the) words are doing?
And what did you do there?
And what did you does?
What did you doed?
What you having?
What me doing to you?
What me fold?
What you have on you nose...what you was
having on you nose?
Sue, what you have in you mouth?

Can I have tapioca?
And you going down with me?
This can't write a flower?
May I whistle?
You have glasses?
Is that some noodles?
Is Fraser having coffee?
Did you drink all you coffee?
You can put these here?
Did you make them?
Do you have pockets in there?
You can put these here?

Stage 3.

ADAM

Where dis goes?
Where she went?
Mommy, where dis want to go?
Where dere's a heel?
Where small trailer he should pull?
Where the man's truck should go?
Where the other Joe will drive?
Where I should put it when I make it up?
Where we went?

Who took them all down?
Who took this off?

Why not...they don't got some teeth?
Why Paul jumping for joy?
Why she standing on (her) hand?
Why he don't know how to pretend?
Why the kitty can't stand up?
Why you caught it?
Why Paul say he going to knock all of my toys down?

Which way they should go?

How he can be a doctor?
How they can't talk?
How my car can't get in?
When this go on?

What it has in it?
What I did yesterday?
What he can ride in?
What we saw?
What Paul trying to see?
What we say oh?
What I can put them in?
What you took out?
What lives in that house, Mommy?
What are wood friends?

Is Mommy talking to...Robin's grandmother?
Are we talking 'bout dat boy burn de school down?
Can they should go down that Mass Avenue?
Did I hit it almost?
Oh, did I caught it?
Do you take it out?
Does it be around?
Did I saw (that) in my book?
Can we be men?
Can't it be a bigger truck?

Do you have games and lessons and stories and bags and anything?
Is Paul gonto be a nurse and I gonto be a doctor?
Will you read about the germs?
I am not... am I a doctor?
Does the car carrier carries that?
Does the kitty stands up?
Did you broke that part?
Ursula, does you tear these off?
Can I make him a tree after a finish this?
Did I see nice things?
Does lions walk?
Does turtles crawl?
Am I silly?
Can I make what I want?
Will you bowl with me?
Do you know what I think it is?

A Sample Grammar

Adapted from Katz and Postal (1964) and Ed Klima (personal communication).

Constituent Structure

$$\begin{array}{lll}
\text{S} & \rightarrow & \text{Q + Nucleus} \\
\text{Nucleus} & \rightarrow & \begin{Bmatrix} \text{NP} & \text{VP} \\ \text{Adv} & \text{Theme} \end{Bmatrix} \\
\text{Theme} & \rightarrow & \text{NP} \quad \text{VP} \\
\text{NP} & \rightarrow & \text{Det.} \begin{Bmatrix} \text{N} \\ \text{pro} \end{Bmatrix} \\
\text{VP} & \rightarrow & \text{Aux} \quad \text{MV} \\
\text{MV} & \rightarrow & \text{V} \quad \text{NP} \\
\text{Aux} & \rightarrow & \text{T} \quad \text{(M)(Perf)(Prog)} \;\; \Bigg\| \begin{Bmatrix} \text{V} \\ \underline{\text{have}} \\ \underline{\text{be}}\ \text{pred.} \end{Bmatrix}
\end{array}$$

Wh can appear with any NP or Adv in S.
If one wh marker, have Q marker as well.

Transformations

T1. Move $\underbrace{\underline{\text{wh}} + \text{X}}$ to left of phrase marker, if S includes Q.

T2. Invert Aux and NP_s if wh dominated by Adv.
Optionally invert Aux but not delete from original position.
Add do to first of unaffixed affix.

T3. Delete Adv where it dominates wh.

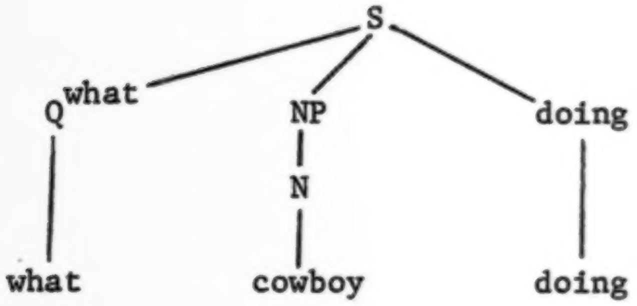
T4. Move Q to right where Q doesn't dominate wh.

T5. Delete Q unless final in sentence. If final, spell as 2 3 3 ||.

__Stage 1__.

Derivation of __What cowboy doing__?

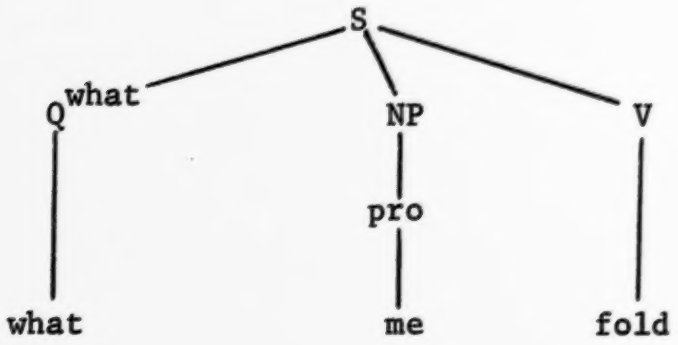
$$S \rightarrow Q^{what} - NP - (doing)$$



__Stage 2__.

Derivation of __What me fold__?

$$S \rightarrow Q^{what} - NP - V$$

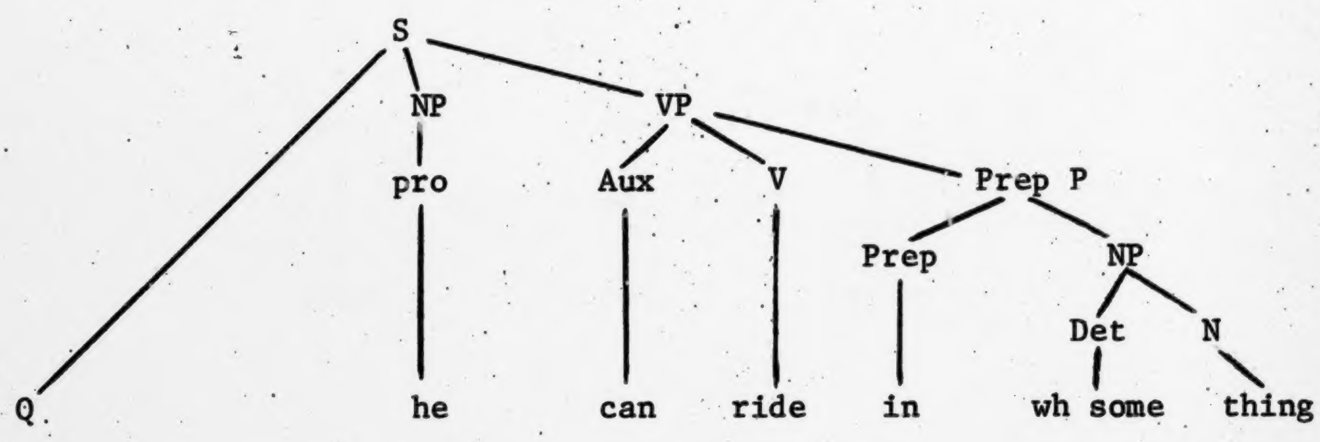


Pro, 1st per. sing. → __me__ in env. wh_____

Stage 3.

Derivation of What he can ride in?

S → Q - NP - Aux - VP



Transformations

T1. Move wh some thing to left of marker.

T2. Does not apply: Auxiliary in ersion is optional rule with wh for children. Word boundary.

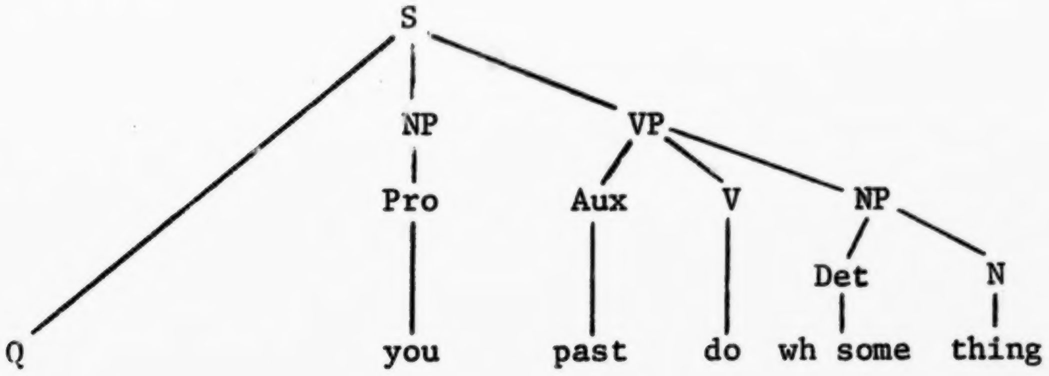
T5. Delete Q. Wh some thing → what.

(following Katz and Postal, 1964)

Stage 3.

Derivation of What did you doed?

S → Q - NP - Aux - VP



T1. Move wh some thing to left of marker

T2. Move auxiliary to left of NP, but do not delete from original position. (Child's rule). Word boundary and do application.

T5. Delete Q. Wh something → what.

(following Katz and Postal, 1964)

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Footnotes

1. This investigation was supported in whole by Public Health Service Research Grant MH 7088 from the National Institute of Mental Health.

2. The group has included Jean Berko Gleason, Colin Fraser, David McNeill, Dan Slobin.

3. Susan Ervin and Wick Miller at the University of California, and Martin Braine at Walter Reed Army Institute of Research.

4. Taken from Roger Brown's study of language samples from the three mothers.

Symbols

(x)	Symbol in parens is optional
$\left\{\begin{matrix} x \\ y \end{matrix}\right\}$	One of the symbols must be chosen
+	Symbols on either side are joined
$X \rightarrow Y$	X may be rewritten as Y

Discussion and Comments: Verbal Structures

Summarized by David McNeill

One remarkable fact about language acquisition is the speed with which it occurs. Beginning with the simple two- and three-word combinations of age 18 months, children develop virtually complete adult grammars by age 3.5 or 4 years--a total period of only 24 to 30 months. Such speed has led some of us to entertain fairly strong hypotheses about children's capacity for language acquisition. Encouraged by Eric Lenneberg's work on the biological basis of language, efforts have been made to examine the idea that children have inborn expectations concerning the form of language, expectations that guide language acquisition and make possible its great speed. These efforts have so far been restricted to the study of children acquiring English. However, one strong prediction based on these views is that the performance of children acquiring diverse languages will show fundamental similarities, even though the languages they ultimately acquire are very different. The theory suggests that the similarities whould be linguistically universal--that is, they should reflect features common to all languages. This expectation is open to empirical investigation. Dan Slobin's paper, which deals with the acquisition of Russian by children, is perhaps the first study attempting to apply such a test.

Children acquiring English, and on Slobin's evidence children acquiring Russian as well, characteristically begin their linguistic careers with a few grammatical classes, with the basic grammatical relations such as subject and predicate, and with hierarchically arranged sentence structures. Thus, they quickly acquire the fundamental structure of their language; but children lack at first what have been called grammatical transformations. Yet, transformational rules are essential to characterize the linguistic competence of adults, and somehow children acquire them. Descriptions of this phase of language acquisition have been completely absent until recently. However, in 1964, Ursula Bellugi described the acquisition of the negative system--itself a complex of transformations--in two of the children in Brown's project. The resulting picture was of intricate but orderly growth of grammatical structure. She now describes the emergence of another system of transformations, this time the rules underlying the formation of questions. The result is again intricate but orderly growth of grammatical structure.

Discussion of Bellugi's and Slobin's Papers

Bellugi's paper was the second presentation of the session, and vigorous discussion followed immediately. Unfortunately, almost none of it dealt with her topic, but rather, with the question of children being endowed with some kind of inborn capacity for language acquisition. A great deal of light was generated by the ensuing debate, but very little illumination. One exception was a question raised concerning the nature of evidence that could be offered in support or refutation of a nativistic hypothesis for language acquisition. This is an important issue. Part of the answer has been incorporated into the introduction to Slobin's paper, above.

Instead of attempting to summarize the remaining 90-minutes' discussion, much of it carried on in tremulous voices, the two statements below, one by Dan Slobin and the other by Wilbur Hass, are recommended. They represent opposite points of view on the issues raised.

Because the conference did not itself discuss Bellugi's paper, the session chairman has added comments of his own in the hope that some of the points she raises will thereby receive consideration. These comments follow.

It is often difficult for psychologists who are not actually engaged in studying the acquisition of syntax to understand why child language should be examined from a transformational point of view. Bellugi provides part of the answer when she writes, "It is...only when we begin to look at what is learned in some depth that we can begin to ask about how language is learned". Nonetheless, there was still considerable confusion on this point at the conference. It seems worthwhile, therefore, to discuss the strategy underlying such studies of language acquisition as Bellugi's.

Implicit in Bellugi's remark is a distinction between linguistic knowledge, or competence, and the process of acquisition itself. These are separate questions and failing to distinguish them would immensely complicate the task of understanding how language is acquired. Consider competence first, for it is here that transformational grammar is directly relevant. We know from linguistics that acquisition eventuates in such knowledge of English as can be characterized by rules of formation, rules of transformation, syntactic categories, etc. The rules underlying questions are examples of transformations;

they state one fragment of an English speaker's competence. However, we do not learn from linguistics the steps that are taken to acquire these rules. For this, we must observe children. The implication of Bellugi's remark, quoted above, is that in so doing, we should consider only those parts of a child's performance that, in fact, lead to transformational grammar. Such a restriction of scope seems perfectly natural. Children do many things, and not all of them are relevant to linguistic competence. To point out, as some did at the Conference, that children's behavior can occasionally be explained by stochastic models clearly contributes little to understanding how transormational grammar is acquired. It is precisely because grammatical competence cannot be represented by stochastic models that transformations are included in linguistic theory (Chomsky, 1957). Nor is it a criticism of Bellugi's method to point out that we do not understand the mechanism whereby children's linguistic performance is derived from their linguistic competence. To do so is merely to claim that Ursula Bellugi should study something other than the acquisigion of grammar, or, perhaps, to deny that child grammar can be studied at all. This appears relevant to the concern, voiced by Hass below, that Bellugi's method is anti-developmental because of uncertainties regarding the manner in which competence controls performance.

The second implication of Bellugi's remark, quoted above, is that it is necessary first to discover what a child learns before reasonable hypotheses can be framed about how he learns it. There can be no question about this assignment of priorities. It is the standard empirical assumption, offering observation as the only effective guard against empty speculation. The same assumption is made throughout psychology, as in all science, and probably it is not necessary to discuss it further.

Bellugi provides a summary of her findings on the development of interrogative structures on p. 125, and it need not be repeated here. Instead, four of these findings have been selected for comment, four that seem to have implications for grammatical development in general.

1. Despite wide variations in input, the milestones of language development are remarkably constant. One implication of this observation is methodological. Longitudinal studies of children are expensive and time consuming. Not only is one paced by the child,

but discovery of the grammar underlying a child's recorded speech is a process often tedious and invariably demanding. Accordingly, the number of children observed is always small. It is encouraging, therefore, that the three children described in Bellugi's paper showed identical stages in the development of interrogative structures. Incidental observations of children elsewhere--a larger sample previously studied by Brown and Bellugi in Cambridge, and the children followed by Ervin and Miller in Berkeley--support the universality of the sequence Bellugi describes.

Aside from the methodological comfort it provides, one may also wonder why children follow so regular a sequence of development. As Bellugi points out, the children necessarily heard different speech samples, and their life circumstances, especially in the case of Sarah, were different. On the other hand, Bellugi also reports that the frequency profiles of various sentence types in the mothers' speech are very similar, and it might seem that this would explain the identical line of development followed by the children. However, such an account clearly is insufficient. Parents use well-formed sentences with similar frequencies, whereas the children show identical sequences of ill-formed sentences. We still do not know how sentence types in partental speech become converted into sentence types in child speech. Some thoughts on this process are contained in McNeill (1965), and it should be pointed out that Lenneberg takes a completely regular sequence of developmental milestones to be one criterion of growth governed by processes of maturation.

2. Comprehension of speech. A bedeviling problem in the study of language acquisition has been to find ways to measure children's comprehension of speech. Tape recordings, obviously, preserve only what a child can produce. But it is likely that children can comprehend grammatical features before they can produce them (Fraser, Bellugi, and Brown, 1963). Moreover, the study of comprehension has the major advantage that the input is known--it being, as one of the linguists at the Fourth Conference on Intellective Processes pointed out, the sentence comprehended (Bellugi and Brown, 1964). In production, on the other hand, the input is completely obscure. The natural technique employed here, of looking at a child's answers to parental questions, is therefore extremely welcome.

The technique can be applied to grammatical constructions of any type, so long as it is known on what features of the question an answer should depend, and it avoids the problem faced in the original study of Fraser, Bellugi, and Brown (1963) of finding pictureable correlates of grammatical features.

3. The widespread appearance of novel grammatical features. The sudden appearance of a grammatical feature in a variety of different contexts is one strong indication of the acquisition of a rule (see Slobin's remarks, below). An example is the way in which the regular past-tense inflection on verbs, -ed, rapidly spreads through a child's grammatical system at roughly age 2.5 years (Ervin, 1964). Slobin's paper contains numerous examples of the same phenomenon occurring in Russian. It is interesting in this light that Bellugi finds the same development in children's use of transformations. In stage 3, auxiliary verbs appear simultaneously in declarative sentences, negative sentences, yes/no questions, and occasionally in *wh*-questions. Before this time, they had occurred in only one environment, negation, and were not introduced by transformations. It is noteworthy that stage 3 is also the time when embeddings, which are constructions governed by transformations of a different sort, first appear, again in a large variety of environments (e.g., *you have two things that turn around*). Add to this Bellugi's earlier evidence that the negative system becomes transformational at this same time, and a picture emerges of children's grammar changing during stage 3, not only in that particular transformations appear, but also in that the general concept of transformation first becomes available. The general notion of a grammatical rule that takes into account a complete phrase structure, as opposed to one that considers only a single item within such a structure, forms the common ground for the auxiliary, negative, interrogative, and embedding transformations. The emergence of a concept as abstract as this should excite wonder in those who believe children acquire only what can be directly exemplified in parental speech.

4. The psychological reality of transformations. Miller and McKean (1964), Mehler (1963), McMahon (1963), Slobin (1963), Savin and Perchonock (1965), and several others have argued that transformations have psychological reality because they operate independently of one another in the memorization and processing of sentences. Savin and Perchonock found that when subjects were required to recall both sentences and a list of

unrelated nouns, sentences and nouns having been presented together, the number of nouns retained was smaller when the number of transformations involved in the sentence was larger. Each transformation evidently occupied storage space, so with a larger number of transformations there was less space available for the retention of other material. These results are from adults. It seems that Bellugi has found somethingsimilar in the speech of children. In stage 3, yes/no questions, wh-questions, auxiliary inversion, and the auxiliary transformation all appear to be part of the children's repertoire. However, it seems that children can perform only two of these transformations at a time. Thus, a child will say can I go out? as well as why he can go there?, limiting himself to two transformations in both sentences. As in Savin and Perchonock's experiment, each transformation appears to occupy a certain amount of "space", and so appears to represent a psychologically real event in the processing of sentences. This finding should be of interest to those who wonder if children process speech in ways fundamentally different from adults (see Hass' comments, below).

Supplementary Statements Submitted by Dr. Slobin

The notion of linguistic "rule" has caused some misunderstanding in our discussion, and should be further clarified. The productivity of language--as reflected in the ability to produce and understand an endless variety of novel sentences--impels one to characterize linguistic competence in terms of rules, whereby the individual can project a limited amount of experience with a limited number of sentences to the capacity to produce and understand an unlimited number of sentences. The use of the word "rule" in this context is perhaps unfortunate, in that it often leads people to think that we believe that children can state explicit rules of grammar. This, of course, is not what is meant. No one can state all of the rules of English grammar. (Indeed, it is this fact which keeps linguists in business!) What I have in mind when I speak about rules in language development is the fact that the child has learned much more than a list of specific word combinations; that he has acquired (or possesses, or has formed) knowledge which makes it possible for him to go beyond the specific collection of sentences he has heard.

Aside from the problem of how to characterize such rules in formal, or neurological, or other terms, there is also the problem of operational criteria for rules of behavior. What sorts of evidence are there to enable one to speak of a person's possession of a rule of behavior--linguistic or otherwise?* I think there are various levels of evidence, from less stringent to more stringent. The simplest sort of evidence comes from analyses of the spontaneous speech of the child. Take, for example, the elementary case of two-word utterances. Already regularities can be detected, since not all possible combinations of units occur. This is the earliest sort of evidence for rules--regularities of behavior.

A more stringent test for the existence of rules is to look for the extension of regularities to new instances. We have such evidence in the spontaneous speech of the child in the case of overregularizations like "it breaked," or "two mouses."

There are, however, even more stringent tests--or definitions--of a rule. On the next level one can ask if the child can detect deviations from regularity--if he can judge if a given construction is right or wrong. This normative sense of rules is a later development in ontogenesis. This is what is referred to as a "sense of grammaticality." Actually, there are several levels of evidence here. The first comes, again, from spontaneous speech. If a child stops and corrects himself, then this is evidence that he is comparing his speech with some standard of correctness--he is monitoring it in regard to his rules. Three-year-olds are frequently heard to stop and correct themselves while speaking.

A more difficult test of this sense of grammaticality is to see if the child can detect ungrammaticalness in the speech of others. At some point, children usually begin to correct each other (and their parents!).

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*Actually, of course, the rules of language are social conventions like many other rule systems, such as etiquette, for example; and I think that what I have to say about rules here applies, in a general sense, to other regular social behaviors as well as to linguistic behavior. The fact that linguistic rules are also cultural norms (regardless of one's position on the degree of biological preconditioning for the acquisition of linguistic rules) makes an explanation of this sort of regular behavior different from explanations of other sorts of behavioral regularities studied by psychologists in artificial laboratory situations--like, for example, the serial position effect in recall. Linguistic rules, like other norms, can be broken.

The most difficult test of grammatical judgment is the direct question. The child can be asked if it is, for example, better, or more correct to say "two mouses" or "two mice." This criterion is not available to the investigator of the early stages of child language.

So far, then, we have the following evidence for rules: We can be fairly sure that a child has some rule system if his production is regular, if he extends these regularities to new instances, and if he can detect deviations from regularity in his own speech and the speech of others. This is generally what psycholinguists mean when they speak of the child's learning, or forming, or possession of linguistic rules.

It is clear, then, that I have left out the most stringent test for the existence of rules--the one that adults always look for--namely: Can the individual state the explicit rule? Using this as evidence of linguistic rules, of course, all of us would fail the test. Since no complete and adequate grammar of English has been written, using this criterion would force us to conclude that linguistic competence cannot be characterized in terms of rules. Clearly, then, this is not what is meant by rules in the context of developmental psycholinguistics.

(It is interesting, though, that after the child has mastered his language he does show an interest in explicit rules. It is well known that children of about six or seven constantly ask meta-linguistic questions. Once they have learned to speak correctly, they generally want to know why, and ask why things are said one way rather than another. At this stage, also, they seem to enjoy playing with rules--breaking and changing them for pleasure--as if they enjoyed their mastery of the rules. So, in a certain sense, the final stage of rule knowlege in childhood is connected with a search for explicit formulations of the rules, and an attempt to exercise full mastery over them. In this sense, then, the adult linguist is continuing along the path of language development.)

Supplementary Statements Submitted by Dr. Hass

Let me say that I am less convinced than some other participants that the "striking recent advances of developmental psycholinguistics" (Slobin) give one the authority to say that previous "psychologists ignored the problem of development of language in children" (Bellugi). It is nothing new to inveigh against forcing the child's speech into adult part-of-speech categories (cf., to name only two, Dewey, 1894; Guillaume, 1927). We do now have more adequate and objective means of saying interesting things about the child's language structure while maintaining safeguards against reading adult structure into it. But these safeguards, based (as Bellugi says) on treating the child as a speaker of an "exotic unknown language," are neither particularly well-thought-out nor foolproof. First, they are not well-thought-out because they are, in one important sense, anti-developmental: To attribute to the child the capabilities of an adult speaker of another language is to overlook that his efforts are derived largely from communicating with speakers of his mother-tongue, and that the way in which a child can be said to "have" a language is probably quite different from that of an adult--it is not just that the child has "different rules," it is that there are differences in the manner and the extent to which the child's behavior is "rule governed." Second, they are not foolproof, because one does not know how to get behavioral evidence relevant to setting up grammatical classes and rules (i.e., grammar writing, as some authorities have taken a certain perverse pride in pointing out, is not completely open to operationalization). Thus, in Slobin's paper, it is not at all clear what evidence could be used to support calling one part of an utterance a "subject" and another an "object." To the extent that this is true, one suspects that the "reading in" of adult concepts may indeed be taking place, and vitiating any talk of universals.

These considerations lead me to suggest another type of language development study which has not been brought up at this conference. As psychologists, we can do more than present general schemata which allow us to synthesize the forms used by a child (or children) at points in development. We can use such grammars to investigate the functional properties of the child's speech. Using our knowledge of the sorts of utterance the child seems to produce as well-formed, we can see how he utilizes specific differences in form in his language activities--we can get at the child's sense of appropriateness of one form rather than another. For example, Bellugi's information on interrogative structures can be used as a basis for studying the child's questioning activity. What differences does the child recognize among the questions he asks? What circumstances lead him to use one question form rather than another? What sorts of responses of others "act as reinforcers" for what sorts of questions? Such studies would help us find out more about what a child is doing when he produces or reacts to a given stretch of speech.

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VERBAL CONTROL

Friday morning, October 22, 1965

David Birch (Chairman)

Verbal control is an important part of the study of the development of language functions in that it focuses on the role of language in the regulation of behavior. The study of verbal control is the study of language in the service of other behavior. Ina McD. Bilodeau approaches the topic as an experimenter who uses language in implementing her experimental manipulations and as a methodologist who wishes us to think clearly and creatively about the issues of verbal control. Charles W. Eriksen has directed a portion of his research energies to the study of learning without awarness (i.e., learning without reportable verbal concomitants) and has extended his investigations into nonverbal behavior. The two speakers served as consultants for Study F: Motivation and Control in the Development of Language Functions (David Birch).

Tangent to Experimental Techniques of Verbal Control

Ina McD. Bilodeau[1]

This paper began as a methodological presentation, suitable to its title, of external, verbal control of behavior--ways in which one person uses words to control another's actions. The preliminary outline included verbal conditioning, instructions, and feedback as familiar topics and knowledge-giving, motivating, and rewarding as familiar functions historically ascribed to verbal controls; it heartily applauded the current flood of interest (and ingenuity) in reapproaching human behavior as human, and therefore verbal.

But somehow the original, experimentally-oriented paper got lost behind a windy aside against wasting this decade's chance to reanalyze verbal behavior, and more, to enlarge our study, on a 1920-style perception. The tangential thoughts took off from a brief consideration of verbal self-control, but were so like those aroused by G. A. Miller's address on psycholinguistics, printed earlier this year in the American Psychologist, that it seemed better to direct my comments to the specific article than to unspecified "undercurrents in contemporary treatments of verbal behavior."[2] So instead of directly offering my views on current trends and significant techniques, variables, and findings in verbal control, I let you infer them from the following summary of a reaction to "Some preliminaries to psycholinguistics" (G. A. Miller, 1965), hereafter called the preliminaries. In further preamble, I acknowledge that formal criticism of the preliminaries would be inappropriate. The effort was a solicited (not volunteered) response to an award, and was prepared for oral delivery before a heterogeneous audience; if the written version seems casual, its writer should not be held seriously accountable but be given credit for trying to instruct and entertain. The overview and later specific comments below are meant equally casually.

The disappointing, over all impression the preliminaries left with me was of blurred issues and truisms. (The atomistic approach is not inconsistent with there being "non-linear sums," or even with a prevalence of non-linear joint effects of variables. Spatial and temporal relations are physically describable, and a relation between variables does not distinguish the psychological from the physical. Some of the failure of present physical-stimulus description to account for behavior--as "psychological"--is that very psychology that Miller scorns.) The disappointment is there because I agree there will be no important questions about speech if we fool ourselves that, except for a few trivial details, all significant language problems have been solved by rat and eyelid. But with its justified complaint against thoughtless and simplified versions of psycholinguistic problems, variables, and interactions, the *preliminaries* makes the same errors about the rest of psychology. If there are two sides, each is guilty of unfairly simplifying order and interaction. In short, I read a fairly standard holistic position, though if the *preliminaries* had been offered in the language (a kind of English) I would have chosen, my apparent agreement would have been greater. At least I fool myself about believing that I am in sympathy with anyone interested in human behavior and that new techniques and methods of analysis can open areas, once hopelessly unanalyzable and unquantifiable, to objective and quantitative treatment. Further, despite a simple-minded bias for reduction, I acknowledge the difference between in-fact and in-principle explanations and know that translating from one area to another may be so distant as to be effectively never, and useless, to boot. (But from my cracker barrel, as opposed to the *preliminaries'*, is added, "So what?" It would be low and sneaking to claim that Miller's opposition asks only wrong questions--*S*-*R* psychology has made more in-fact progress than alchemy, and is not all that way off.) Some of the objection below may be evoked by the Gestalty flags the *preliminaries* unnecessarily waves; clearly many portions are taken approvingly. The *preliminaries*, however does not admit so much as an in-principle relation between learning and using language, though Miller repeatedly makes a major implicit concession to his chief antagonists; at least one can interpret much of the *preliminaries* to mean that psycholinguists deal with stimuli that have already received a full course of discrimination training. It is not stretching a point very far to describe Miller's paper as largely a complaint about the fact that human beings show transfer of training in using language.

The *preliminaries* covers each of seven aspects in three sections; the comments below are grouped under the second treatment ("Some Implications for Research") but include statements from the first ("A Point of View") and second ("A Critique") treatments without distinguishing the source. If complaint far outweighs applause, it must be remembered that my intent is to complain about unfortunate items in a generally laudable revival of interest in human behavior. The preliminaries was selected as containing examples that fit my complaints, not for a comprehensive pro-and-con review of its contents, and even less as an evaluation of its writer's other words.

1. "Not all physical features of speech are significant for vocal communication, and not all significant features of speech have a physical representation."

The title of this aspect is round, ringing, and nonsensical, and more appropriate to parapsychology than to psycholinguistics. The first half, taken as nonexplanatory, is okay; how a feature of speech gets to be significant or not significant, however, is a matter well within the range of inquiry of Miller's archenemies, the students of discrimination learning. The second half is not okay; at best the lack of physical representation means that speech, as one psycholinguist wants to begin with it, includes a great deal of learning, concept formation, etc. (i.e., general psychology)--his unquestioned privilege, though the choice could be stated more accurately and less irritatingly. This is a new context for the standard claim that one must know how the subject perceives a stimulus in order to predict (to me, other) behavior in a given situation. This kind of statement is blind to the fact that the antecedents of this same perception are the object of other people's inquiries.

At least two issues are being made fuzzy under the first aspect. a) As elsewhere in the preliminaries, order, sequence, context, and relation are labeled psychological, although one can specify physical relations between events (where and when they occur, and even where and when, relative to each other) and must specify them for a complete physical description. b) Miller's predictor seems to be other people's dependent measure; the somewhat hypothetical population of discrimination-learning psychologists ought to crow just as loudly that, "...physically identical utterances can be treated differently..." And I understand the preliminaries to offer here as explanation, that speech perception implies grouping and interpretation (something taken for granted as part of the definition of perception).

2. "The meaning of an utterance should not be confused with its reference."

Here the preliminaries pushes two different senses of significance (a "...central and unavoidable concept..."), reference and meaning, partly in order to scrap conditioned vocalization as having nothing to do with meaning, but at most only with reference. This section is by and large neutral for psychologists who can agree, for example, that "...meaning...depends on intersymbol relations..." Examples of referentless sentences and a rough indication of the limits of what makes a referent, however, are unfortunately not included. (Incidentally this seems to be a confusion crying out for psycholinguists to reconcile psychologists and linguists, with their different vocabularies.)

3. "The meaning of an utterance is not a linear sum of the meanings of the words that comprise it."

Of course not. But what is a linear sum of meanings? Also, "...studying the meanings of isolated words..." may not be so dreadfully limited as here implied. To show how context affects a word's meaning (or, "...how words in a sentence interact...") aspect 3 offers fountain pen and play pen as different pens, though "...phonologically and orthographically identical..." (Is it really necessary to point out that a, b, and c does not mean a + b + c?) The single-word people would have writing, baby, cattle, and jail associations in their collections; the preliminaries' illustration is far better as an example of context eliminating alternative meanings than producing brand new ones--and more to the point is how the two get to be pens and how they get to be different pens. What kind of trouble would a person have if he had just one of the associations? Would the preliminaries advise helping out the unfortunate foreigner by pointing and saying or by having him generate a multitude of sentences about pens? (Could I talk in France of putting the sheep in la plume? And, why don't I generate more French sentences. Surely I am as human as if I were French.)--The "Venetian blind" vs. the "blind Venetian" is fair in neither grammar nor psychology, neither of which ignores order. Even in the low-down lab concerned with (rat) discrimination learning, there is attention to organization and sequence of events, and much fuss, for example, over whether a certain click comes before or after food. (There are other little word games to illustrate all kinds of points--for example, in the lack of meaningful relation between such expressions as "on hand" vs. "on foot", and Tom Sawyer's natural and unnatural sons.)

I hear the assertion that compounds cannot be handled from component meanings, but nothing convincing is offered that it is fruitless to search for elements, wholes, and combining rules.

4. "The syntactic structure of a sentence imposes groupings that govern the interactions between the meanings of the words in that sentence."

This must be granted, as well as that syntax in turn depends upon syntactical environment This aspect and the next might well be accepted as a statement of the _preliminaries'_ central interest in understanding language use. Other investigators with similar central interests (in the transfer of training family) in higher processes, problem solving, concept formation, etc., are able to perform rather similar efforts without kicking psychology on the way.

5. "There is no limit to the number of sentences or the number of meanings that can be expressed."

This is a necessary point (skill vs. rote) and one that the oversimplifying non-linguist may overlook in explaining language behavior.

That we cannot learn an infinite number of sentences also provides a two-pronged attack on _S_-_R_ learning approaches, or perhaps upon all psychology,that a) learning is a minor matter and b) we must change our hypothetical constructs if we want to handle language. Oddly, some of the reasons the _preliminaries_ offers against are what others would offer for using words before sentences. Otherwise a lengthy comment or nothing seems required for the fifth _aspect_. It probably never hurts those engrossed in studying learning to be reminded that we want comprehensive behavior theory in which learning plays only a part, and not always the biggest. Various approaches to theory makes sense, and we may end up with a variety of different theories and even, perhaps, different kinds of theories--questions affect answers, and what we set out to explain bears on what we do explain.

6. "A description of a language and a description of a language user must be kept distinct."

Pious assertions aside about the usual custom of keeping clear the distinction between the subject (human or rat) and his lever, what the _preliminaries_ distinguishes is that psycholinguists need to know both rules (or "knowledge"), which they get from linguists, and performance. It seems that psycholgy would expect that _preliminaries_ to take the rules as part of the stimulus, but they are not so handled.

7. "There is a large biological component to the human capacity for articulate speech."

This truism does not advance our understanding without some hint of how we are "...tuned by evolution to select just those aspects that are universally significant..." What is the mechanism of this tuning and what are the universally significant aspects of language? A brief explication of these would not be out of line with the level of sophistication of the rest of the preliminaries; otherwise I prefer the plainer "speech is human", accepting _aspect 7_ as preliminary to the preliminaries.

Summary

Let me repeat that much in the _preliminaries_ is unarguable, and I am pleased to find so human an emphasis upon one of man's two most human and important abilities. I am convinced, for example, that chaining is not all of language--and less than that of language use, and we owe credit to psycholinguists for adding language use to psychology. No preliminary justification is required. We could put in a complaint too, for going beyond the _preliminaries'_ range, as well as short of it. Words do not carry all the meaning, and the _aspects_ omit facial and vocal expression (which can make or reverse meaning, everything else constant). Finally, again, events are always ordered, not just events in language, and any psychologist takes order into account: the staging of stimulus and response events is the first item of a psychologist's business. None of us has more claim to peculiar sequential effects than another, nor any greater reason to expect interaction.

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Footnotes

1. This paper was prepared while the writer was on research leave at the University of Washington. The support of a grant for leave of absence from the University Council on Research of Tulane University is gratefully acknowledged.

2. Present-decade approaches to perception such as Attneave's, however, are welcomed, and the line between other- and self-controlled behavior is imaginary. As Birch (1965) has said better, explanatory effort may be devoted to laws relating overt initiating stimulus and terminal, overt response, or to such mediating internal events as plans and hypotheses. In problem solving and concept formation (which step on the toes of any area of human behavior), the internal goings-on matter a great deal, and the (within-subject) *R-R* chain between external, experimenter event and measured, external, subject event may be long and complex. But many people (Bourne, E. A. Bilodeau, C. E. Noble, in my own area of interest, for a few) have for years been having an objective look at just such subjective things as hypotheses. One further admission, or elaboration of Birch's point above, is that either an *R-R* or *S-R* approach is reasonable. To measure what the subject is (his perception) and predict from it has all my approval--all that is needed is the measuring. The Language Development Program gives the happy impression of wanting to cover the whole range: general laws of learning, individual-difference laws for known antecedent conditions, and individual differences as controllers of other behavior.

Subjects' Hypotheses, Experimental Instructions and Autonomic "Conditioning"

Charles W. Eriksen

Learning and/or conditioning in the human adult does not occur to any significant extent in the absence of verbalizable cognitive concomitants. I have reached this conclusion on the basis of not only my own research but in very important measure upon the extensive work of Dulany and Spielberger. They and their associates have intensively and systematically investigated verbal operant conditioning and in my opinion their results demonstrate quite unequivocally that verbal response modification does not occur in the absence of the *S*s' ability to define verbally the response-reinforcement relationships, at least at the level of correlated hypotheses. Further, Dulany's work strongly suggests that verbalizable knowledge of response-reinforcement contingencies is, in itself, not sufficient to produce behavior change. The *S*s must also verbalize an intention on their part to change their behavior in the direction of reinforcement.

It is true, of course, that the work of these investigators has dealt exclusively with operant conditioning of verbal behavior. And the very fact that the operant response is itself verbal may somehow prevent its blind or automatic shaping by reinforcement without the *S* being "aware" of the process. However, in my laboratory we've been similarly unsuccessful in producing operant conditioning of skeletal nonverbal responses without the *S*s' awareness. Nor have we been successful in getting *S*s to learn to use an extraneous but correlated cue on perceptual tasks without their being aware of the nature of the cue.

In one experiment (Paul, Eriksen, and Humphreys, 1963) we attempted operant conditioning of face, hand, or foot movemements through the use of a strong primary reinforcement. The *S*s were placed in a heat-humidity chamber and while engaged on an irrelevant task, were reinforced with a 10 sec. draft of cool air to the face and neck upon the occurrence of a preselected movement. While a certain proportion of the *S*s showed "operant conditioning" they were without exception those *S*s who were able to verbalize the relationship between their behavior and the reinforcement. The remaining *S*s did not differ significantly nor appreciably from a control group who received the same number and distribution of reinforcements but where the reinforcement was not contingent on any specific response.

Following Brunswik's (1943) theorizing on perceptual learning and perceptual constancies, we have attempted to obtain learning of perceptual contingencies in human adults in the absence of their verbal awareness of these relationships (Eriksen and Doroz, 1963). In one experiment Ss were asked to judge which member of a pair of colored lines was longer under the ostensible purpose of studying color illusion on line length. Unknown to the Ss one color when it occurred was invariably associated with the longer line of the pair. Despite a large number of acquisition trials Ss, who at the end of the experiment were unable to verbally state the association of color and line length, showed no use of the color cue in their judgments in the instances where the cue color line was paired with a line of equal length. On the other hand, subjects who could verbalize the contingency showed a significant and appreciable bias in judgments for the line of the contingent color when confronted with pairs of equal lines. Similar results were obtained in a second experiment where a supposedly extraneous but correlated structural cue was associated with affective judgment of stimuli.

While operant conditioning of verbal and nonverbal behavior and even perceptual learning of correlated cues does not seem to occur in the human adult in the absence of some type of cognitive mediation, the area of conditioning, particularly of autonomic responses, would intuitively seem more promising. In fact, experiments by Diven (1937) and Haggard (1943) have been widely interpreted in text books as having demonstrated unconscious conditioning of anxiety.

Lacey and Smith (1954) have pointed out some serious methodological and analytic errors in both the Diven and Haggard studies, deficiencies which render a conclusion of unconscious conditioning extremely equivocal. In an experiment designed to correct the methodological deficiencies, Lacey and Smith administered a chained word association procedure to their Ss in which the stimulus word list contained a number of repetitions of the word 'cow' and the word 'paper'. One group of Ss was always administered an electric shock following chain association to the word cow and the remaining Ss were shocked following their chained associations to the word paper. Anticipatory changes in the Ss' heart rate that occurred during associations were analyzed for evidence of autonomic conditioning relative to verbal awareness. In their study Ss were classified as unaware if they were unable to verbalize the specific contingency between shock and the word cow (paper). Their questioning of Ss on this point, however, may not have elicited all the information available to the Ss' verbal report. It is quite possible that the Ss if asked, could have given several or more words which they were quite confident were not shocked. It seems reasonable to assume that the Ss' change of heart rate to a stimulus word which he is confident will not be followed by a shock will be less than to words of which he is still unsure (potentially dangerous). It also would seem reasonable to expect the actual shock word in general to be among those that S was uncertain of rather than among the words he was rather confident were not followed by shock, while a semantically dissimilar word would more likely be in the latter category.

The evidence that previous studies have found on conditioned generalization to words in the same semantic classification as the shocked word might actually represent not generalization so much as a beginning on the part of the S to learn to discriminate the general area where shock occurs. Thus Ss in the cow shock group would tend to classify rural words in the uncertain category and nonrural words, including paper, in the confident not-shocked category. The reverse would occur for the paper shock Ss. In view of this possibility it becomes apparent that the control measure used to determine whether conditioning is occurring becomes quite crucial.

In the Lacey and Smith study conditioning was computed by taking each S's heart rate score to cow and subtracting from it his heart rate score to the word paper. It should be readily apparent that this method of computing conditioning is apt to lead to a spurious appearance of precise discrimination by heart rate. To show that conditioning of heart rate is more specific than the stage of the S's verbal awareness, an analysis is required that demonstrates whether on not the S shows greater autonomic conditioning to the actual shock word than he does to the other words to which he verbalizes uncertainty as to whether they were followed by shock.

We repeated the Lacey and Smith experiment in its essential details (Chatterjee and Eriksen, 1960) using the GSR rather than heart rate as the autonomic response and more intensively investigating the Ss' verbalizable hypotheses concerning shock. At the conclusion of the experiment each of the stimulus words was read to S and he was asked to rate how confident he was that he had or had not been shocked to this particular word. When the Lacey and Smith method of classifying awareness was used we were in general successful in verifying their result provided their method of computing conditioning was employed. However, if GSRs to the shocked stimulus word were compared with the Ss' GSRs to other words he thought he had been shocked to or might have been shocked to, there was no evidence of conditioning. The GSR was no greater to the actual shocked word than to other words the S verbalized as being potentially dangerous. In other words, the Ss' GSRs were no more precise in discriminating than were his verbalizable hypotheses. In fact the two showed a high degree of correspondence.

Our conclusions from the above study are in close agreement with those of Branca (1957) who carried out a conditioning and generalization study of the GSR with shock as the UCS. He concluded "expectation of shock as a painful or fearful experience was necessary and sufficient to produce responses to the experimental and generalization stimuli in this experiment and such expectancy was the result of awareness of the existing relationships between the experimental stimuli and experience with the unconditioned stimulus" (p. 549).

In the above experiments the Ss' hypotheses or expectancies were allowed to develop during the experimental procedure. The effect of a S's cognitive expectancies upon autonomic conditioning is even more dramatic when they are experimentally manipulated before the conditioning procedure. In the next experiment (Chatterjee and Eriksen, 1962), Ss were asked to chain associate to a twelve-item word association list. The list was repeated over seven times, for the conditioning trials, each repetition a different random ordering of the words, and three more repetitions of the list were given during extinction. The Ss were assigned to one of three groups. Group I, the informed group, were told prior to conditioning that an electric shock would follow one particular word but that no other words would be followed by shock. They were further told when the extinction trials began there would be no further shock. Group II, the partially informed Ss, were told that following the presentation of a particular word in the list would always be a shock and that each of the remaining words in the list would be followed by one shock sometime during the trials. They were further told that during the latter part of the experiment all the shocks would cease. Group III, the uninformed Ss, were told that a certain number of shocks would be administered at certain points in time during the experiment but that E could not tell them beforehand when the shocks would come. These different instructions for the three groups were designed to lead to different cognitive expectancies concerning conditioning arrangements. In keeping with these differences in instructions Group I Ss were given only seven shocks while Ss in Groups II and III received 18 (seven to the word boat and one each to the remaining II words presented during the conditioning trials). During conditioning and extinction trials heart rate was continuously monitored with a Grass Model 5-P4 polygraph.

Following the conditioning and extinction trials all Ss in Group I were able to verbalize readily that shock had followed only the critical word boat. The Ss in Group II who had received a total of 18 shocks were also able to readily verbalize that they had discriminated the word boat from the remaining words by the third or fourth presentation of the word boat. On the other hand, none of the Ss in Group III were able to verbalize clearly and unequivocally that shock had followed boat nearly all the time during the conditioning trials and occurred only once to each of the remaining words. However, a post facto attempt was made to subclassify the Group III Ss based upon the frequency of reported shocks to the word boat relative to the other II words in the stimulus list. Group IIIa consisted of those Ss who reported they thought they had received three or more shocks to the word boat and not more than two shocks to any of the remaining words. Group IIIu lacked even this minimal discrimination.

A rigorous test of the precision of conditioning is to plot the percent of Ss in each experimental group who gave the maximum positive cardiac response difference to the word boat in each block of trials. The results of this measure are seen in Fig. 1. It is

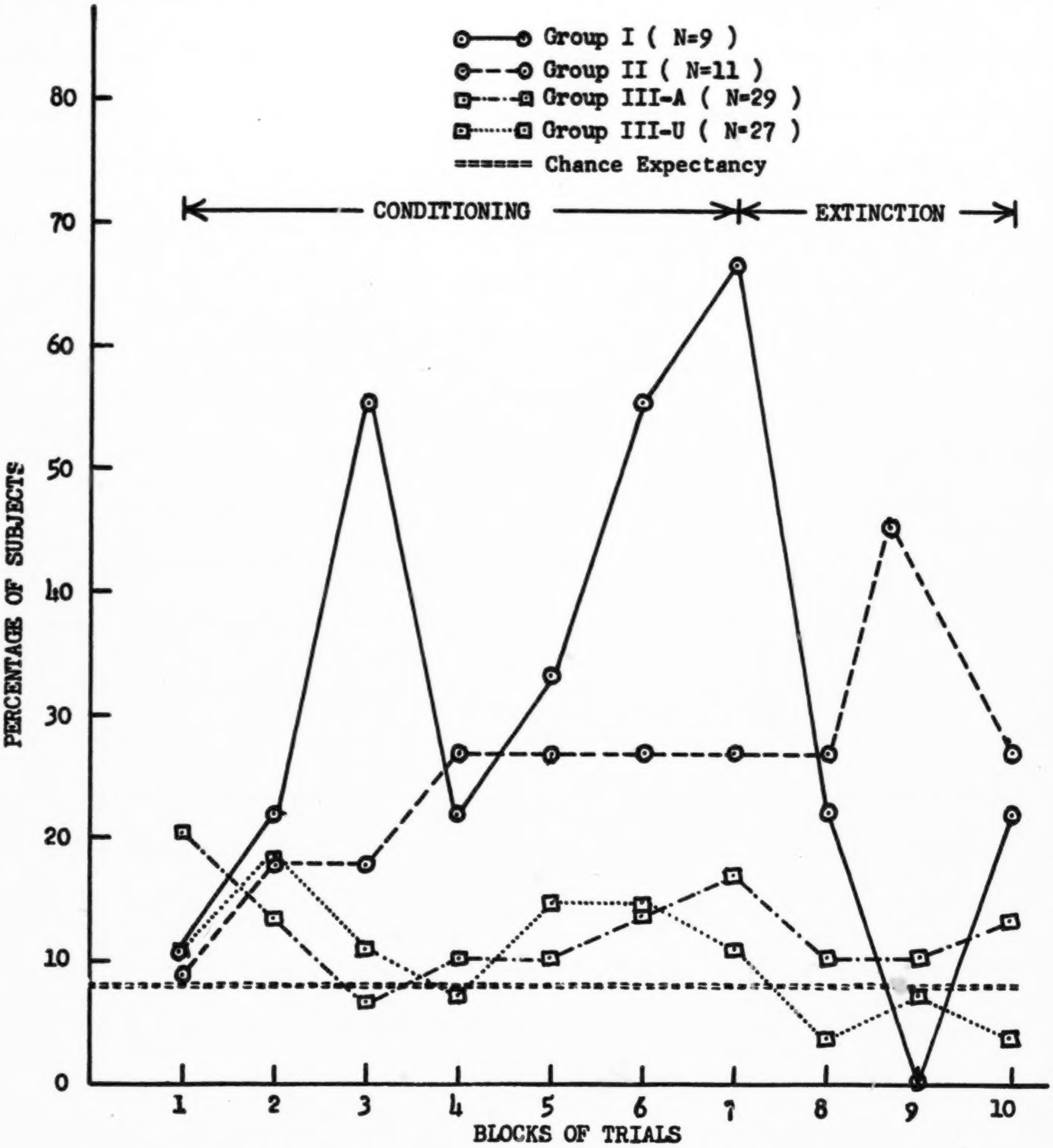

Fig. 1. Percentage of subjects whose "maximum response difference" follows the stimulus boat.

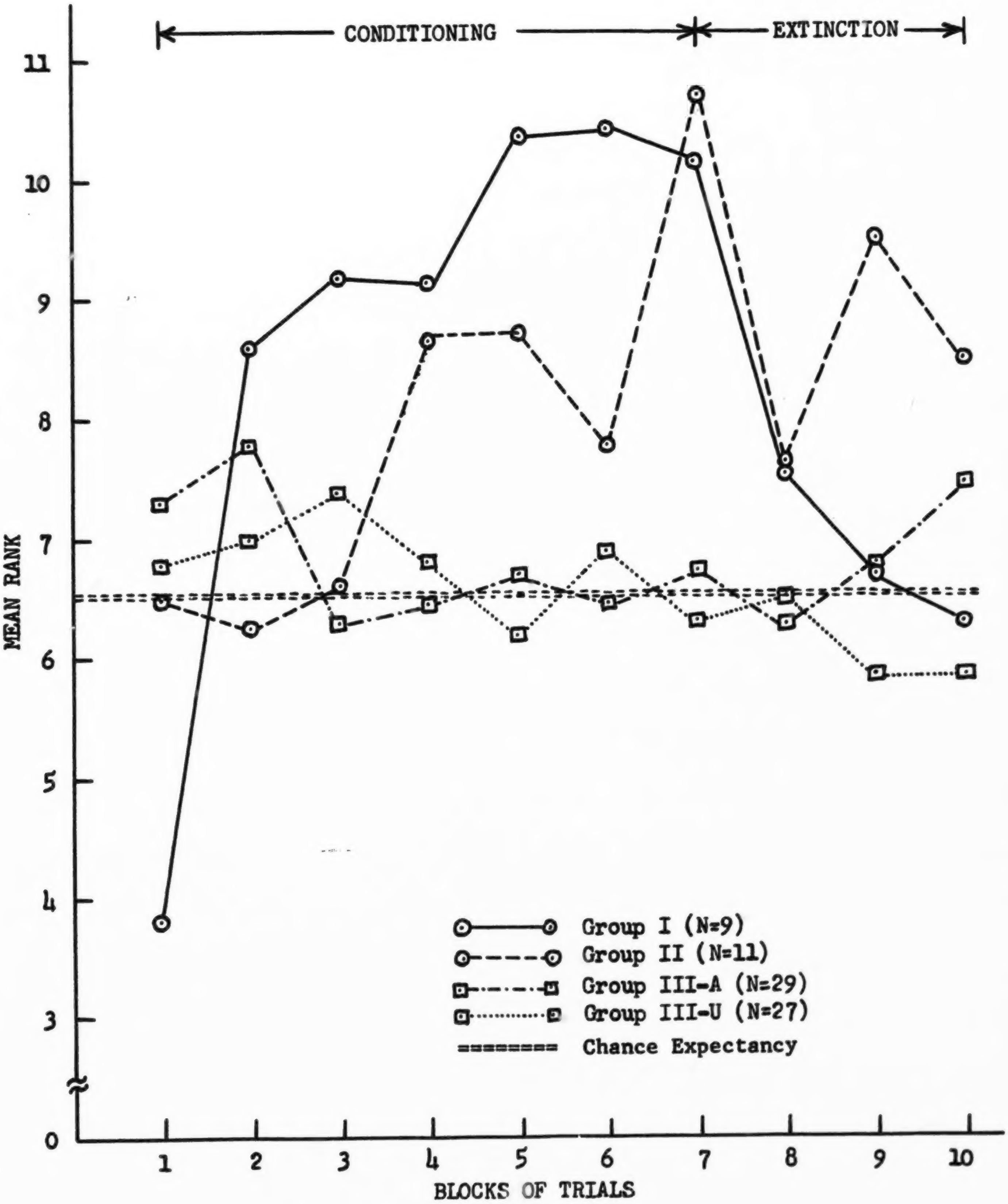

Fig. 2. Mean rank of "cardiac response difference" which follows the stimulus boat.

apparent that the conditioning varies from group to group. It's greatest in Group I, somewhat less so in Group II, and in Groups IIIa and IIIu the curves indicate that no conditioning occurred.

A somewhat less severe criterion of conditioning yields essentially the same results. Here the magnitude of the cardiac response to each word in a trial block was rank-ordered from small to large for each S and then the rank value for boat was averaged through Ss by group and trial block. Curves obtained in this way are shown in Fig. 2. Again Groups I and II show evidence of conditioning whereas Groups IIIa and IIIu do not.

The data in Figs. 1 and 2 are also quite informative concerning cognitive expectancies upon extinction. As will be recalled, the Group I Ss were informed prior to beginning the extinction trials whereas the other two groups were not. As is seen, extinction is present in the Group I Ss on the very first extinction trial as opposed to the noninformed Ss in Group II.

These results show quite clearly the effect of information or instructions given to Ss upon their heart rate responses to the various stimuli. We can ask a reverse question as to whether differences in heart rate behavior will predict Ss' verbalizations. To answer this question the data of the Group III Ss are available. The Ss in this group were asked to report the number of shocks they thought they had received to each of the stimulus words during the experimental trials. Our previous analyses suggest that words having a large cardiac response difference should have a higher number of reported shocks than words with a low cardiac response difference. To test this possibility the word giving the greatest cardiac response difference on the seventh conditioning trial was selected for each S along with the word producing the smallest cardiac response difference. The number of reported shocks to these two words was determined for each S in Group III. The mean number of shocks reported to the word with greatest cardiac response difference was 1.37 as compared with 1.03 for the word with the smallest difference. A t-test for correlate scores gave a value of 1.71, significant at the .05 level for a one-tailed test.

The remaining experiment I have to report is of classical conditioned discrimination using a high and a low frequency tone as the CS and/or neutral stimulus, the GSR as the conditioned response and electric shock as the UCS. Each S experienced 12 occurrences of the CS and 12 of the neutral stimulus during conditioning in which the CS had a duration of 5 secs. and the UCS was presented .5 secs. after cessation of the CS tone. The Ss were randomly divided into an informed and an uninformed group for the conditioning trials. The informed Ss were told that during the experiment as they sat in the sound-treated room, they would be exposed to two different tones on different occasions. The high (low) tone would always be followed by shock whereas the low (high) tone would never be followed by shock. The uninformed Ss were merely told that they would be shocked during the experiment and their task was to sit as quietly as possible in the experimental apparatus.

In Fig. 3 the GSR response in conductance units is shown to the neutral stimulus and to the conditioned stimulus summed through the 12 conditioning trials by informed and uninformed groups. Both groups show a greater GSR to the CS than to the neutral stimulus but the difference between responsiveness to the neutral and conditioned stimulus is much greater in the informed group. These Ss show less reactivity to the neutral stimulus than do the uninformed Ss and greater reactivity to the CS.

In Fig. 4 GSRs to the CS and neutral stimulus are shown for the informed and uninformed groups as a function of conditioning trials. Both groups show a general adaptation of the GSR as a function of trials but the informed group is the only one that shows a clear difference in responsiveness to the CS and the neutral stimulus. In fact, the largest difference between CS and neutral stimulus response occurs on the very first trial for these Ss. However there is still an appreciable difference in GSR for the Ss in the informed group on the last conditioning trial.

Upon completion of the twelfth conditioning trial, half the Ss in each of the two groups were informed of the reversal between the CS and the neutral stimulus. In other words, the informed Ss were told that from then on the previously neutral stimulus would now be followed by shock whereas the previous CS would no longer be shocked. Figure 5 shows the effects of this reversal of conditioning as a function of whether or not the Ss were informed of the reversal. As is seen, when the results are summed through the 12 reversal conditioning trials the Ss who were informed of the reversal show a clear

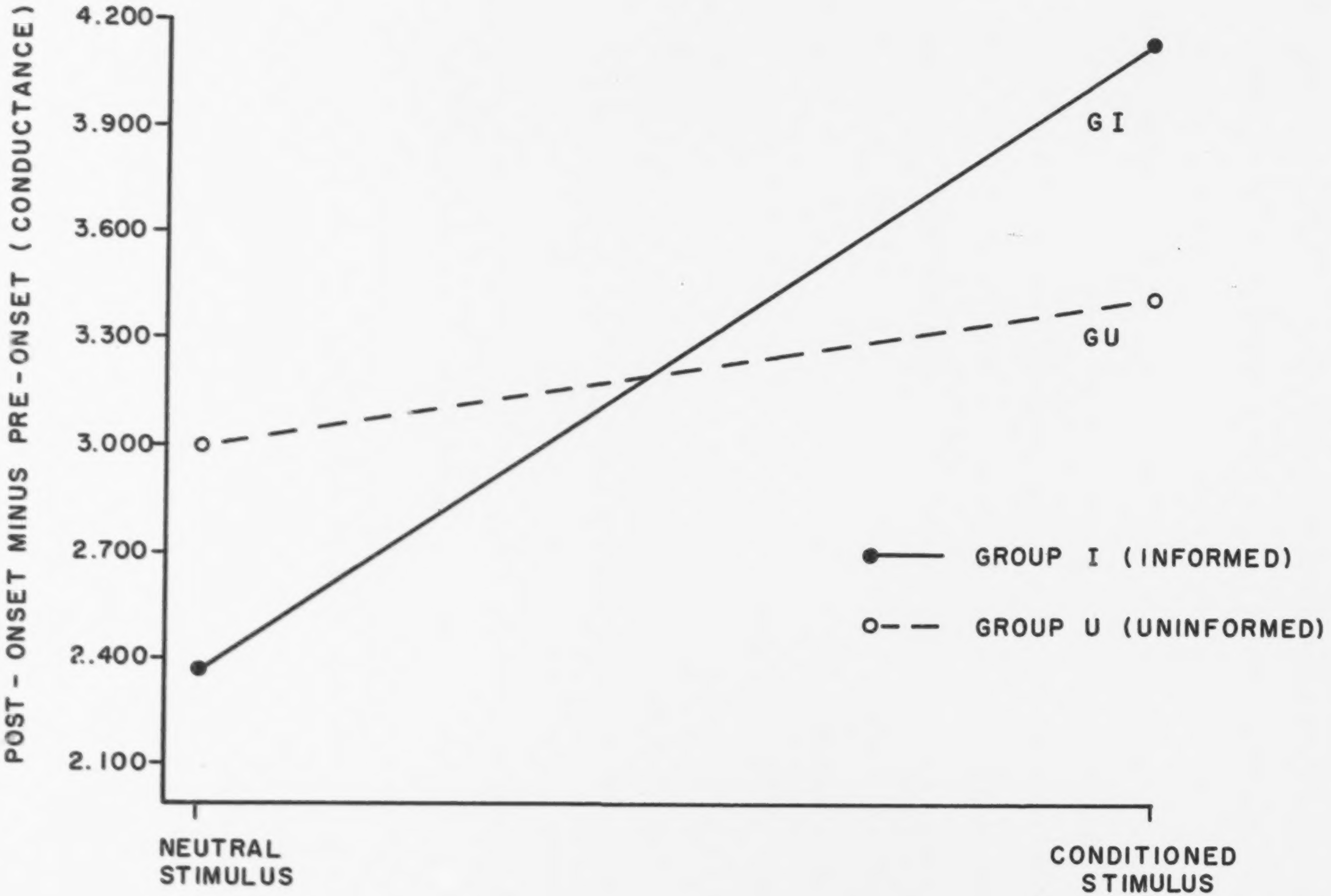

Fig. 3. The GSR to the neutral stimulus and conditioned stimulus summed through the twelve conditioning trials by informed and uninformed groups.

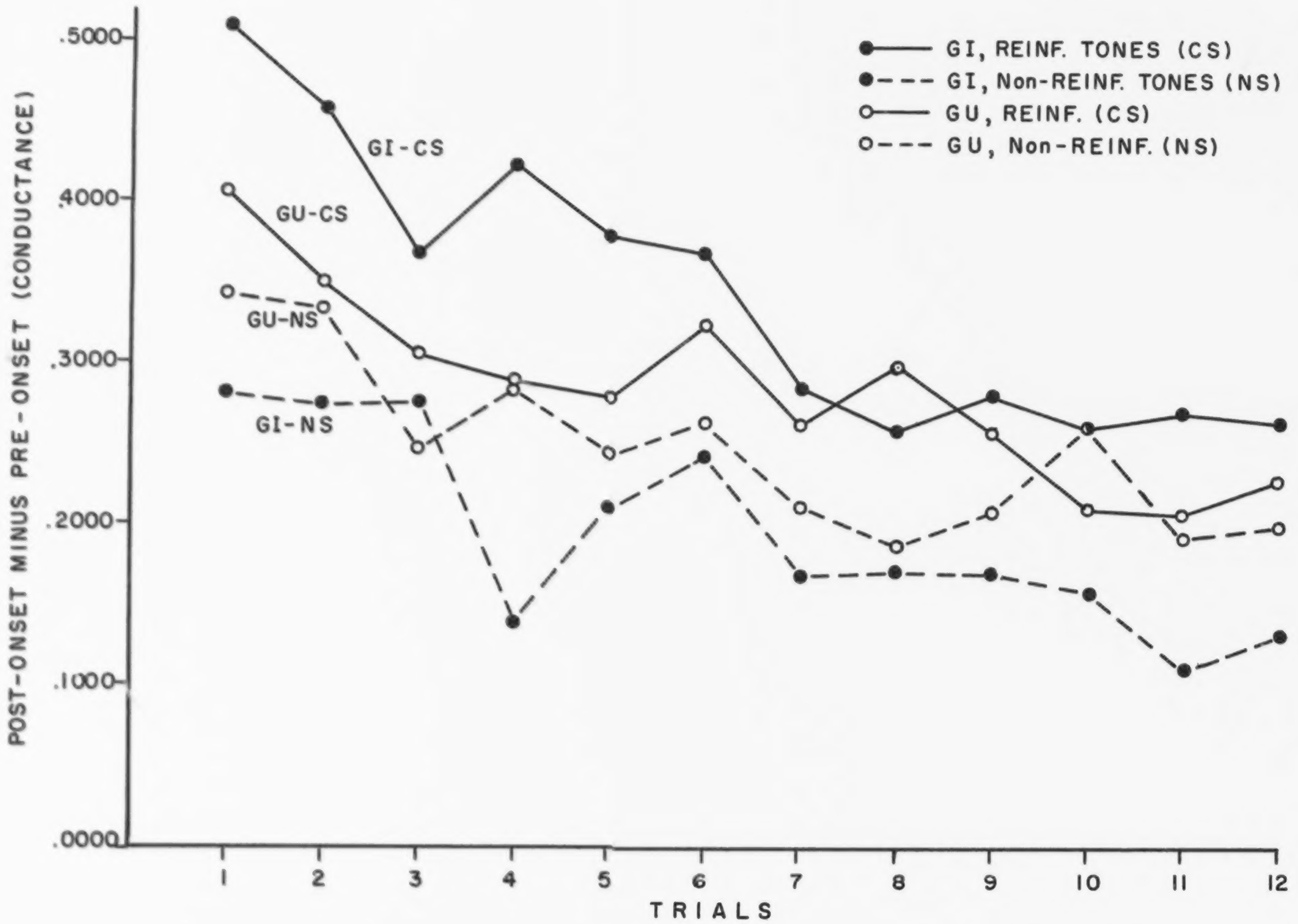

Fig. 4. The GSR to the conditioned and neutral stimulus for the informed and uninformed groups as a function of conditioning trials.

discrimination between the now neutral stimulus and the new CS. The uninformed Ss show no evidence of a discrimination. They also show a much greater reactivity to both stimuli than do the informed Ss.

In Fig. 6 GSRs to the neutral and CS are shown for the informed and uninformed Ss for each of the twelve reversal of conditioning trials. The results are quite similar to those obtained for conditioning. Again, informing the Ss results in a clear discrimination in GSR responses between the CS and neutral stimulus, a difference that is apparent on the first reversal trial and which persists throughout the twelve trials. The uninformed Ss on the other hand show little or no evidence of a discrimination between the two tone signals. They react with appreciably greater GSRs to the neutral stimulus than do the informed Ss which might be anticipated since in keeping with their lack of information, they would tend to perceive this signal as potentially shock-producing.

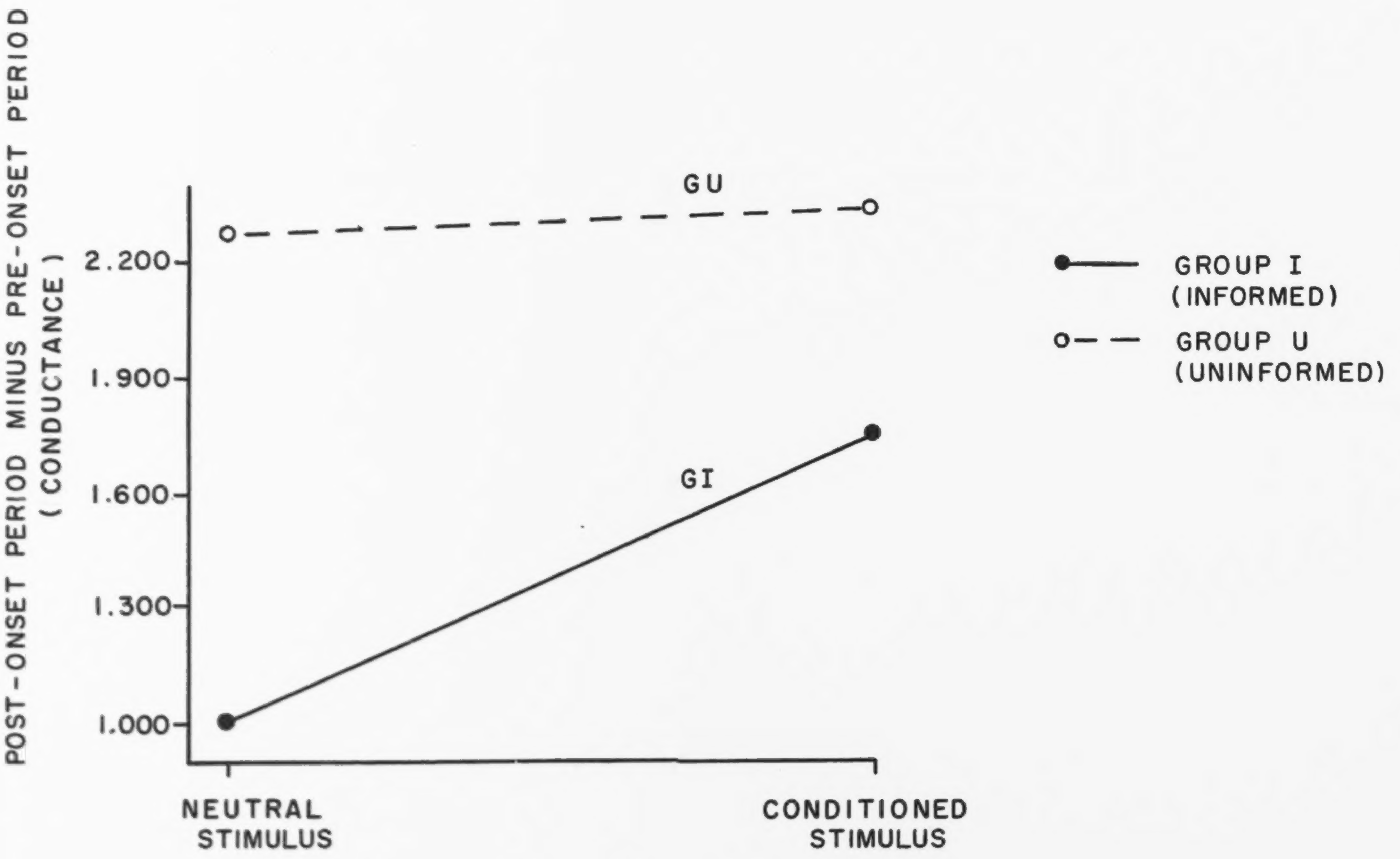

Fig. 5. The effects of reversal of conditioning as a function of whether or not the subjects were informed of the reversal.

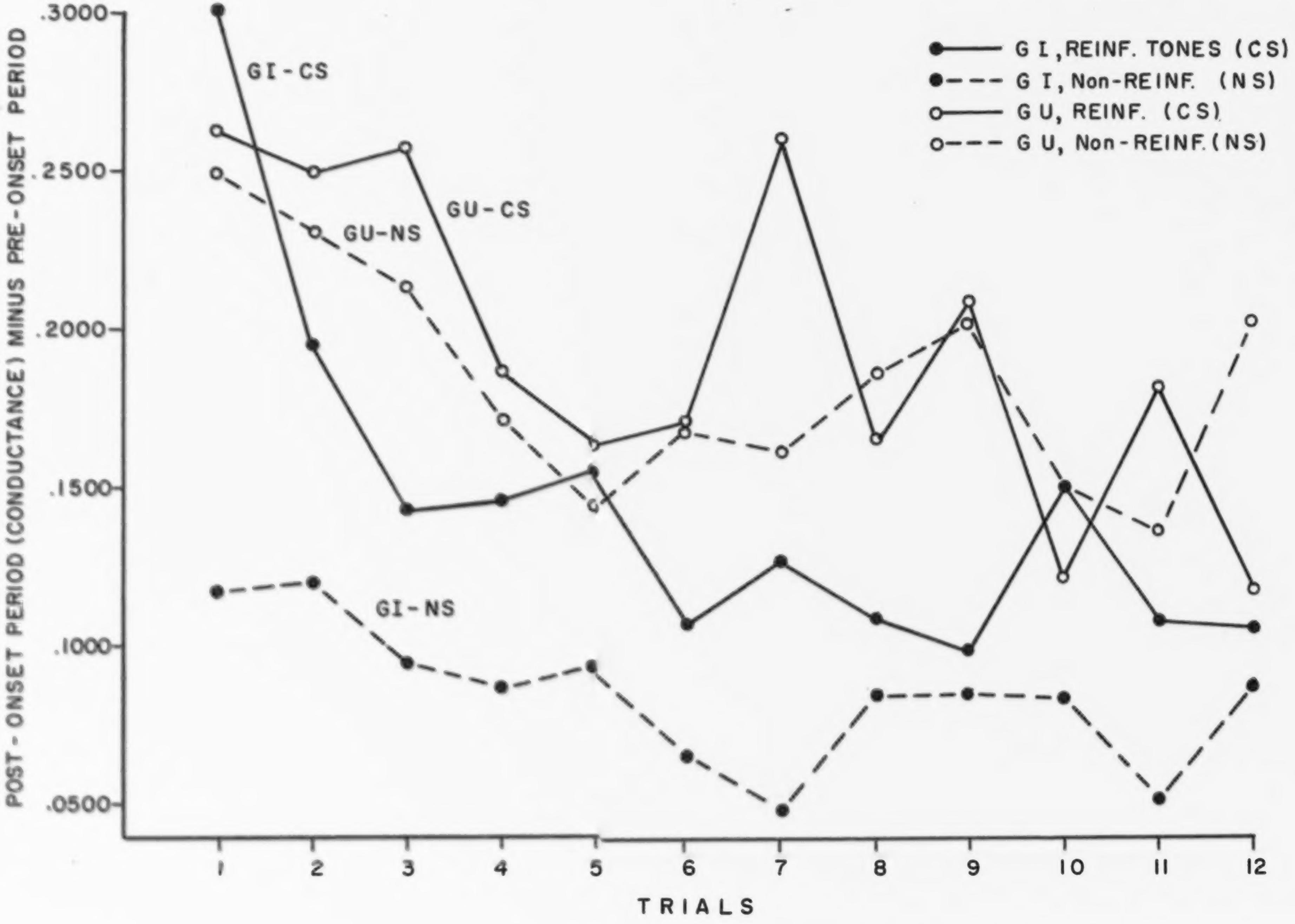

Fig. 6. GSR to the neutral and conditioned stimulus for the informed and uninformed subjects for each of the twelve reversal of conditioning trials.

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Discussion and Comments: Verbal Control

Summarized by David Birch

1. Central to a discussion of verbal control is the recognition that the acquisition of language is a topic different from the utilization of language. The study of verbal control begins with an organism that has some degree of verbal facility and thus the principles of language acquisition are not our primary concern. Conceptions of how language is learned are not irrelevant to our interests, however, since it is likely that they will color the way we look at how language is utilized. If we conceive of the basic process of language acquisition as the formation of stimulus-response connections, we shall probably think differently about verbal control than if we conceive of language acquisition in terms of the learning of rules. I'm not at all sure that this dependence in our thinking need be so, but I think it highly probable that it is so. In her paper Dr. Ina Bilodeau speaks directly to the qestion of the form we would have our theories of language functions take. Her concern is that important theoretical issues be kept separate from those that do not really matter, and her plea is that we not relive earlier days of confusion and conflict.

2. As Dr. Ina Bilodeau states, external verbal control of behavior occurs when "one person uses words to control another's actions." In everyday behavior external verbal control is exercised, or at least attempted, each time we instruct, command or even request someone to engage in some activity. In the laboratory instances of verbal conditioning, instructions and feedback are, of course, common. We have a great deal of practical knowledge, both as inhabitants of the world and as experimenters in the laboratory, of how to make use of external verbal controls and of which devices are most effective. We are only beginning, however, to theorize about and to do research on the process that operates when one person's words control another person's actions. We know that telling someone to do something works (at least sometimes), but what events occur in the receiving organism over time to yield the resulting action? The latter question is the focal one for the study of external verbal control.

Another form of verbal control exists when we instruct ourselves. This might be called internal verbal control to indicate that the source of the command is in the receiving organism. We are all aware of instructing ourselves on occasion ("watch the ball", "slow down", etc.) so it seems clear that instances of internal verbal control do occur. One wonders if the process of internal verbal control is the same as that of external except for the source of the command or are the two systems only superficially similar? Further, we know far from enough about the developmental sequence that carries the nonarticulate child, who is presumably without internal verbal control, to the self instructing adult. The Russian effort on this problem has been extensive and ingenious (Luria, 1961) and has provided important information and new techniques of study. Their research has suggested to them that the developmental sequence for verbal control is from external command to self produced but overt command, to internal command. Are they on the right track?

3. Dr. Eriksen deals directly with an aspect of verbal control in his paper and he summarizes his findings by saying, "Learning and/or conditioning in the human adult does not occur to any significant extent in the absence of verbalizable cognitive concomitants." This is a precise statement, well supported by the data presented. In fact, the evidence seems strong enough to make it useful to state two questions implicit in Dr. Eriksen's careful statement: (1) Does the verbal mechanism play a causal, antecedent role in learning and conditioning in the human adult or is it only an accompaniment to or even a consequence of the learning and conditioning? (2) While learning and/or conditioning in the human adult does not occur to any significant extent in the absence of verbalizable cognitive concomitants is it also true that previously acquired responses are not performed without verbalizable cognitive concomitants? We lack adequate evidence on this point but observation suggests that (a) some activities are performed independently of verbal functions (b) some with no accompanying verbal functions but with subsequent verbal recall (c) some with concomitant verbal description and (d) some with direct self instruction. All possibilities are of interest and importance, and it is probably more fruitful to investigate each systematically than it is to argue for any one as universal.

SUMMARY AND FINAL COMMENTS

Eric H. Lenneberg

Eric Lenneberg is so well known for his work in psycholinguistics that no special introduction is necessary. His breadth of activities in such diversified areas as comparative linguistics, the biological bases of speech and speech pathology, and problems of language, thought and culture, make him particularly well suited as a general discussant and consultant to our Research Program-Project on the Development of Language Functions. The following are his final comments on our meeting.

Summary and Final Comments

Eric H. Lenneberg

I am grateful to Klaus Riegel for allowing me to have what I have always wanted to have since early childhood: the last word. During these three days there have been several heated discussions on various issues and I shall try to summarize these. I can promise you that I shall do this with as little bias as if a political rally were to be summarized by the late Senator McCarthy.

I have been wondering whether the papers presented at this conference constituted a fair cross-section of work in the U. S. that might be called psycholinguistics. There appears to be a hierarchy of relevance to the main topic of the conference, that is to The Development of Language. One may divide the papers heard here into six categories. The first category would be the least, and the sixth category the most relevant one.

First let us look at the papers that are only indirectly related to language. The relationship is essentially based on untested hypotheses. For instance, the untested hypothesis may be "language learning has a probabilistic element." From this proposition it may look as if all studies in probability learning were automatically relevant to language But this, of course, is not so, and in fact there are many psychologists and linguists who will not even accept this hypothesis. I must stress at this point that the authors who presented papers on probability learning (Bogartz and Weir) were very explicit in denying any relevance to language, and their conclusions and inference were sound enough and modest at the same time. My comments on relevance, in this as in the next instances, are my own personal aside.

In the second category there are papers that are also only indirectly relevant to language, the reasoning being roughly the same as in the previous category, but the difference is that in this group the experiments involve at least a verbal response. Examples here are the experiments in word association. The contributions by Ed Bilodeau and Palermo are cases in point. Neither of the authors made claims about relationship to language.

In the remaining four categories we now approach the phenomena of language more directly. In the third category I would group papers such as those by Eriksen, in which it is shown that language habits are an important factor in man and influence other cognitive processes. In this case the research involves language but one does not inquire how language develops, but merely demonstrates that once it has developed it cannot be disregarded. Nevertheless, such demonstrations are new on the psychological scene and have contributed considerably to our knowledge concerning the relationship between language and other psychological events.

The fourth category concerns direct observations on human behavior which is vocal but is not yet language. Examples here are the papers presented by Markel, and Lane and Shepard. The study of infant vocalization is certainly of great interest to anyone concerned with language but we still do not yet know what the transitional stages are between those vocalizations heard during the first year of life and those which are recognized as verbal behavior in later years.

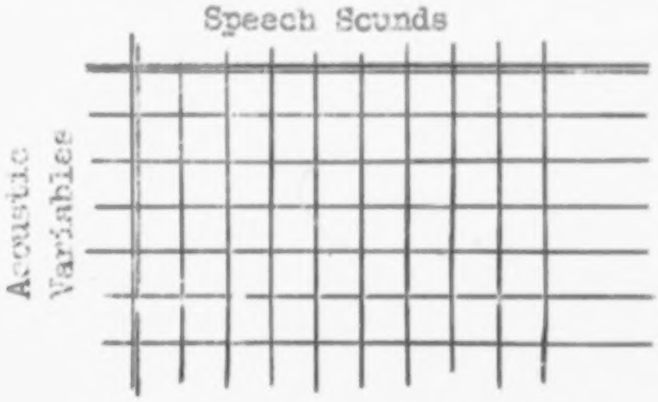
Category five is comprised of presentations in which secondary sources are analyzed. However, the articles surveyed are all direct observations on language development and are thus relevant to our main topic. At this conference Slobin and Mrs. Bilodeau are good examples of this.

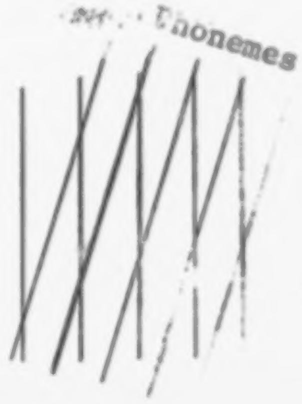
In the last category belong those papers in which direct experiment or observations are performed on language development in children, and the results here are of greatest importance. It is interesting (and at the same time gives us cause to pause) that at this conference only one paper belonged in this category, namely Mrs. Bellugi's, and this excellent paper has not been discussed in full detail during the discussion period following her presentation. I must concede that we (myself included) are to be blamed for this failure in that we began to talk about rather irrelevant issues at the time of Mrs. Bellugi's discussions instead of concentrating on the very interesting points which were made in her paper.

In this day and age we pride ourselves on our objective approach to the study of the nature of man by which we frequently mean to imply that we can, nowadays, look at the behavior of man in exactly the same fashion in which we would look at the behavior of animals. How far from the truth this is! Consider the scientific discovery of a communication system among a certain species, say the honey-bee. How different would we approach this phenomenon from research on man! Instead of assuming knowledge of all the mechanisms involved in the transfer of information, we would actually start by asking "What is it that is being transferred from individual to individual?" "How is it accomplished?" "How does the individual bee perceive the message?" "How is this behavior acquired?" As long as we deal with bees we would never once hesitate to ask whether this type of behavior is learned, under what circumstances it is learned, what role environment plays upon its learning, or whether it is innate and under what conditions it may be inhibited. Yet when we raised these questions during this conference in the context of man it was generally felt that the questions posed were inappropriate, were of a metaphysical nature, belonged to the philosophy of science, or ought to be ruled out of order altogether. It is my belief that this reflects an attitude on our part that is not conducive to the complete exercise of the search for knowledge.

I would now like to make a few comments on the notion of simplicity. Very frequently we hear that a behavior such as language is not researchable because more elementary knowledge is still missing. We are told that until we know all the details of this more elementar knowledge we cannot proceed to work on the more complex problems. An analogous type of argument may be heard among biologists. For instance, the biologist working on intra-cellular processes may feel that it is impossible to study the function of an organ if we do not yet even know how a single cell works. Those working on organs may feel that the interaction of organs cannot be investigated unless we know more about the function of the component parts and finally those who work on physiological interaction of various organs may still object to our research on behavior where we are frequently in the dark even as to which organs are involved. However, I do not believe that there is a logical beginning in science. It will be centuries before the knowledge that accrues from research on a single cell may be usefully applied to the problems concerning interaction of large cell colonies and the same holds true for other levels of operation. Any scientific investigation is bounded by ignorance on the more complex and on the more elemental levels and these boundaries have not, in the past, constituted limits to fruitful investigations. When we are dealing with behavior we do not know indeed what constitutes elements of complex behavior patterns. For instance, it is not at all certain that knowledge of memory, or knowledge of stimulus generalization, or knowledge of pattern recognition, gives us direct leverage for the understanding of the operation of such a complex pattern as language. Each behavior pattern has its own peculiar ways of combining or making use of the various faculties such as memory and recognition, and I consider it to be perfectly proper to work immediately and directly on the behavior pattern as a whole without first waiting for all the information to become available concerning the constituting cognitive processes.

The problem of elemental knowledge and simplicity may be directly applied to language. It is very often thought, particularly by teachers of the deaf, that language acquisition consists of acquisition first of the elemental segments, namely phonemes, and when the child has mastered the phonemes of English he then learns to put these together into words and then to associate words with things. After this has been mastered the child supposedly proceeds to learn to make phrases out of words and sentences out of phrases and language is established. Nothing could be further from the actual state of affairs. In fact, the very definition of a phoneme makes it quite clear that this concept is not even applicable to a pre-linguistic stage. A phoneme is essentially a pattern of contrasts but a pattern which has a function within a larger unit, namely the morpheme. Most of you are familiar with Jakobson's distinctive feature matrix.





Each acoustic sound may be characterized by a value of a given acoustic variable called a feature; in the matrix the feature variables are shown as rows. The speech sounds appear as columns. Now a phoneme is not isomorphic with speech sounds. I have drawn the column for phonemes slantwise to indicate that we may have a single phoneme which, however, may be acoustically like one sound in one context and like another sound in another context. Phonemes change their physical acoustic characteristics in accordance with the context in which they appear. Sometimes the speech sound is modified in expectation of a speech sound which is yet to come, while in other circumstances a phoneme is modified by a prior phoneme. Thus, the child's task is not at all the gradual accumulation of individual sounds which later on he learns to put together but instead it is the differentiation of complex contrastive patterns which are regulated by rules that may well be designated " a syntax of sounds." This problem has been particularly stressed in recent years by those people who are engaged in machine speech recognition and whose aim it was to build a typewriter that would print out letters operated entirely by spoken language received through a microphone. Such a typewriter cannot function unless it has stored within it a complex set of rules governing the phonology of English.

Much could be said also about the problem of the acquisition of reference, but since time is running short I shall confine myself to a few remarks concerning problems of meaning of sentences as against the meaning of individual words. Consider the following four sentences

1.) Joe loves Mary.

2.) Mary loves Joe.

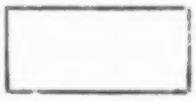
3.) Joe is loved by Mary.

4.) Joe, who is loved by Mary, doesn't love her.

Here it is obvious that we cannot understand the meaning of the sentences unless we can analyze them grammatically. There is clearly no simple rule which says that the first noun is to be the subject in a sentence. Sentences 2 and 3 have different subject-object orders, but mean the same, and sentences 3 and 4 have the nouns Joe and Mary in the same sequence but mean different things. If the child were to learn nothing but the meaning of individual words he would not be able to understand the language spoken around him. He must immediately endeavor to learn certain rules for grammar.

For the remainder of the time I should like to show that rules of grammar belong to a very large class of rules - let me call them rules of correspondence - with which psychologists are very familiar. Allow me to transgress for the purpose of illustration.

Below I have drawn a number of views of a certain object. Each view shows the object from a different angle of regard and the general sequence illustrates that there is a visual rule of correspondence that relates Fig. a to Fig. d. The rule of correspondence here belongs to the set of rules known as shape constancy rules. There is a whole family of similar rules and there are a number of similar configurations that are physically different and yet relatable by a subject in terms of one or another of these rules of correspondence. In the fig. below I have drawn a Necker Cube. It is well known that this is an ambiguous figure. We may either see it as if it were above us or beneath us and we may explain the ambiguity by saying that it may either be related to a figure such as shown to the left or to one such as shown to the right. It is clear that the ambiguity arises by invoking either one of two possible rules of correspondence, each tracing the ambiguous figure to a different origin.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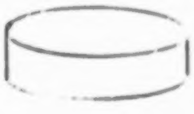
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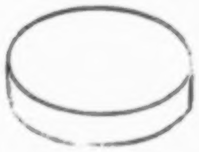
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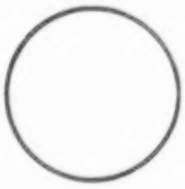
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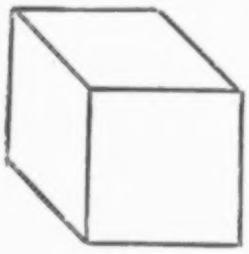

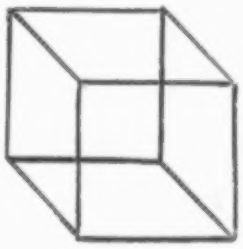

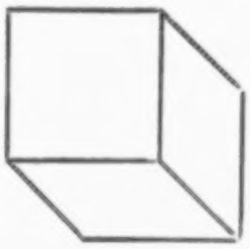

Rules of correspondence obtain also in the realm of temporal patterns. For instance, a melody can be changed by a rule of correspondence, transposed as it were, to any one of a number of different pitches, or we may play it faster or slower and apply a number of specified transformations without impairing the recognition of similarity between these melodies. We also see how there can be rules of correspondence in which both temporal as well as spatial configurations are combined. For instance, a horse has different modes of locomotion. I am not a horseman myself but I believe he can walk, trot, canter, gallop. Now, if we number the feet and watch the sequence of footfalls it becomes obvious that the sequence of footfalls is reordered in at least some of these forms of locomotion.

Take one final example: once we have learned to perform a pattern of movements with the right hand we are capable of transferring this skill to other limbs without further practice. This and the example of gait implies that the schema for the pattern is a central, neural phenomenon. It must not be confused with the peripheral execution itself. On the periphery very different muscles and neural commands are involved if we circumscribe a circle (by drawing into the air) using the right hand or the left foot. Yet we can see and feel a connection between the two patterns and that connection enables us to transfer the skill from arm to leg.

And now I come to my final point. I would like to propose that the rules of grammar are essentially of the same formal nature as the rules of correspondence that we have discussed so far. Take the sentence:

They are boring psycholinguists.

Now this sentence is ambiguous and the reason for it is quite simple. We may either link the words "boring psycholinguists" into a phrase or we may link the words "are boring." Therefore, the predicate in this sentence may be construed as having either of the following two structures: the verb phrase may be seen as consisting of two elements or the noun phrase may be so construed. Notice how this is analogous to the Necker Cube situation. The ambiguity arises by imputing to one and the same stimulus configuration one of two interpretations, or which is the same, one of two rules of correspondence. Each rule leads to a different pattern. The patterns of grammar here are called phrase structure patterns and the ambiguity may be characterized in terms of either of two possible tree diagrams, such as drawn customarily by the generative grammarians.

Now let us take the phrase:

The shooting of psycholinguists.

This phrase is ambiguous even though its general phrase structure does not explain the ambiguity. There is only one possible tree diagram here. The ambiguity in this case arises from the fact that such a phrase may be related by us (grammatically and cognitively) to either one of the following:

Psycholinguists shoot.

and

Psycholinguists are shot.

For each of these sentences we can postulate a rule in which the elements are transformed in a specified way. Since the sentences are different in structure, the transformation rules would be quite different in each case, but when applied to their respective sentence, a phrase results which has one and the same structure in each case. It is this convergence which causes grammatico-perceptual ambiguity.

This latter example of ambiguity is also, in a sense, analogous to the Necker Cube but the rules of transformation in the footfall of the various gaits of a horse in that we are dealing here with a rearrangement of specific events in time.

The point of this discussion is very simple. Grammatical rules are not so very different from other rules which seem to be part and parcel of cognitive function of all animals. They are in the nature of rules of correspondence. They are merely a formalization of our ability to perceive similarity between physically different stimulus configurations and vice versa.

When we are dealing with rules of correspondence in the perceptual world we are quite willing to accept the notion that the capacity for applying rules of correspondence has something to do with the biological nature of the animal. If it has been claimed in recent years that there may be something innate in our capacity to acquire syntax we are probably making no stronger a claim than that man is applying some rules of correspondence to a peculiar type of human behavior, namely verbal communication. This in turn, as I believe has already been suggested in our discussion led by Eriksen, may simply be a reflection of generally human types of cognitive mechanisms.

EPILOGUE

Judging from the replies of many participants, the two and one-half days of discussion have been useful, though few, if any, of us arrived at the meeting with identical expectations. Some participants were primarily concerned with the development of language, favoring the viewpoint of one or another modern linguist. Others felt more concerned with related, and sometimes only remotely related, areas subsumed under the wider and thus ambiguous topic: The Development of Language Functions.

Repeatedly, the meeting made us aware of the danger which any field as young as the field of psycholinguistics is bound to face and which psychology itself has faced during its early history, namely, the danger of dogmatism. There is a natural inclination in such fields to subscribe to a theory so strongly that one tends to judge all evidence and all other theoretical interpretations in the light of that theory. Many of the participants questioned such an attitude and regard it as a revival of the old fallacy reflected in Hegel's reply to his critics, "If nature does not act as my theory predicts, it is very unfortunate for nature".

The confrontation of these two attitudes repeatedly caused the discussion to shift into general problems of the philosophy of science with emphasis on theory construction, the role of theories for sciences and the testing of theories. While the majority of the participants was far from denying the significance of theories for psycholinguistic explorations, considerable disagreement remained as to the relationship between theory and research. One group of participants tended to regard a theory as a "true" model of some psycholinguistic competence; the other group felt that the dualism implied in such a distinction created problems much in the same way as the dualism of Descartes created the mind-body problem. They felt that only when we speak of competence (as opposed to performance) are we facing the problem that certain aspects of performance are prior to experience and cannot be attained through training (nativism vs. empiricism) and that we regard certain aspects as being at the base, others at the surface (genotypes vs. phenotypes).

Implied in the distinction of competence and performance, and the preference for the former, is the claim that such a linguistic theory fulfills functions like those of mathematical or logical systems. This is a reasonable claim if, indeed, it were rigorously applied, that is if linguistic theories were viewed as conventional, formal systems that claim no real-world validity. Unfortunately, many linguists as well as many psychologists fail to take this step. Instead they claim intrinsic ties between their concepts and rules and psychologically "real" events. Thus, their theories become not only meta-linguistic, but meta-psychological and meta-physical.

If I were to summarize the opposing attitude to such views, I would emphasize that a theory is a tool created for a certain purpose, whose value is to be judged by its usefulness, completeness, and simplicity. At our present state of science, and of psycholinguistics in particular, no theory should be regarded as a standard for the evaluation of all experience and considerations should not be limited to any single theory. Of course there will never be a hard and fast rule on the mutual dependence of theory and research, but we all would like to prevent either our theory or our research from becoming a servant of the other. Moreover, the history of psychology, especially of clinical psychology, has taught us the danger of relying on the interpretations of one or a few persons, particularly if those were based on clinical (add: linguistic) intuitions alone.

The present writer did not object to a discussion of these topics and regards their explication as a crucial step for the understanding of a field that overlaps with many other scientific disciplines and that is therefore not uniquely defined in its research strategy. Undoubtedly, many of the participants would have preferred more intensive discussions of special problems. If this has not been always possible it is hoped that the present report has lent itself to such specific explorations.

December 10, 1965 Klaus F. Riegel